

Young Scientists are Distinguished Too

DISCERNING viewers were presented with a rare choice between the channels last week. ITV was showing the cliff-hanger of a soccer international between England and Poland; a match which England needed to win to proceed further in the World Cup—and did not. BBC was simultaneously showing a live programme in its *Controversy* series; a debate on the subject 'Soviet Scientists; a cause for concern?' In terms of action it was no contest. There was no real spirit to the rather formless debate until the last couple of minutes when Sir George Porter interrupted the reading of a quite interesting Communist apologia to substitute Sir Robert Robinson (as mistimed as Sir Alf Ramsey's substitution of Mr Kevin Hector at Wembley).

Nonetheless the debate, however cool and remote, did leave one clear feeling; that the British scientific hierarchy is elderly and does not convey a very lively impression of the state of British science. This remark is made in complete isolation from the question of who said what.

Even though the Royal Society did not choose to send along any of its officers, it cannot easily avoid responsibility for this state of affairs. Since it claims to be the national academy of science for the United Kingdom, it must be held ultimately responsible for the health of science, and thus for the adequate management and representation of science and scientists of all levels in both national and international affairs. This it cannot do adequately within its present framework, because it lacks anything approaching a reasonable leavening of younger scientists. This is not to deny the need for elder statesmen. It is to assert that there is an ignored and large constituency of younger men and women. These, particularly physical scientists, do their most imaginative work in the ten to fifteen years following graduation and that is the time when the best need every support that they can get, political, administrative and financial. This undoubtedly includes recognition by learned societies and most of all the Royal Society. Yet this recognition tends to come when the bloom of youth has worn off—almost as if the society waits until there cannot be a shadow of doubt that the work done is sound. Soundness seems to be a prized quality—one is reminded of the qualms of Nightingale in *The Masters* that Crawford might be elected to the mastership and then have his science discredited.

Anyone can have pleasant sport with the omissions from the list of fellows. Every scientist, of whatever training, seems to find plate tectonics fascinating at present, but he should not look to the fellowship for much of a discussion of it, at least at first hand. Black holes fare a little better. Inadequate representation is not a misfortune only of younger scientists. For instance, no-one seems to be a fellow by virtue of his ability to communicate science to laymen or schoolchildren or even, some may cynically say, to undergraduates. In short the society is far from being representative of the present scientific scene which is so diverse in its character. The policy-making, particularly on such issues as international

collaboration, is in the hands of distinguished but at least middle-aged academics. No doubt a defence can be made that the society is greater than its fellowship in that it sponsors numerous committees and some excellent meetings, and also that many of its fellows have diverse interests and so in a sense cover the missing ground. All this is true and yet it is inescapable that at the centre of the operation of the society is the need for a representative fellowship.

What remedy is there for the inadequacies? The Royal Society has a monopoly in speaking for British science. It chooses who and who not to have as its membership. It is unlikely that any government is going to have the time to impose any reform on it and so the pressure must come from practising scientists both inside and outside the society. It would be better if the moves came from within and it seems inescapable that the only way to reform it is to increase the fellowship substantially. There are doubtless excellent administrative reasons why the society cannot bear a substantial increase in its number, just as Oxford and Cambridge colleges have sound administrative reasons (such as a lack of suitable lavatory facilities) for not wishing to go co-educational. One hopes that discussion does not hinge on these reasons. If the backlog of worthy candidates could be reduced rapidly, more conscious efforts could be made towards creating every year at least a portion of fellows in their late twenties and early thirties. There are plenty around who would add distinction by their election.

100 Years Ago



Publication of Learned Societies' Transactions

IN *NATURE*, vol. viii. p. 506 Mr. Röhrs wishes that our learned societies would publish their papers separately. I have urged this before in *NATURE*, but unsuccessfully. With Transactions such as those of the Royal Society, the present system is almost an absurdity, for papers on most incongruous subjects are bound up together, and the cost is too great. When once a paper is printed, the Council seem to think that there is nothing more to be done, and do not in any way try to make the work known. All papers should be sold separately as cheaply as possible, and on publication, should be advertised in the scientific journals.

If this were done, we should not have men like Prof. Sylvester writing as follows:—"I owe my thanks to M. Radau and the editor of the *Annals of the Ecole Normale Supérieure* for having been at the pains to disinter the little known conclusions contained therein from their honourable place of sepulture in the *Philosophical Transactions*."

W. B. GIBBS

From *Nature*, 8, 550, October 30, 1873.



OLD WORLD

Bad Blow for Optical Astronomy in Britain

PROFESSOR MARGARET BURBIDGE has resigned as Director of the Royal Greenwich Observatory. The announcement last week came as no great surprise to the astronomical community in Britain, but the news of her car accident on Saturday, the day after her resignation, was received with dismay. Fortunately, Professor Burbidge's injuries were restricted to a broken arm and multiple contusions and she was expected to be discharged from hospital in a few days, it was learnt earlier this week. Her husband, Professor Geoffrey Burbidge, was in California at the time but it was not deemed necessary for him to fly to Britain.

Margaret Burbidge will relinquish her position at the RGO on November 30 and will be succeeded by Dr Alan Hunter, the present deputy director at the observatory. She will then return to the University of California at San Diego where, once again, she will team up with her husband, who remained a member of the faculty there when his wife came back to Britain in the summer of 1972.

The resignation has caused despondency among British astronomers. Dr Hunter said this week that it was "a terrible pity that she had resigned, for there were high hopes that Margaret Burbidge would have made a successful director".

When Margaret Burbidge took up her appointment as director at Herstmonceux fifteen months ago she only took leave of absence from her faculty position at San Diego. It has always been clear that she might go back to California if the directorship of the RGO proved unsatisfactory.

According to the Science Research Council, Professor Burbidge resigned because she prefers to return to her own research rather than devote a large part of her time to administrative matters. But in an interview in *The Daily Telegraph*, reported before her accident, she put a slightly different slant on her resignation. She is reported as saying that she is the wrong person in the wrong job and that part of the trouble "has been the inability of the Science Research Council to provide a set-up where my husband and I could work together".

Several efforts have been made, however, to bring Geoffrey Burbidge back to Britain. Last year he was offered a specially created post at the Royal Greenwich Observatory at a salary of £7,500 which compares most favourably

with the top of the professorial salary range. But he declined. He was also offered a chair at the University of Sussex, which is not too far from



Professor E. Margaret Burbidge.

Herstmonceux where he could have carried on with his work in theoretical astronomy, but this he also declined to accept. It is difficult to see how greater efforts could have been made to attract him back to Britain.

Margaret Burbidge, according to *The Daily Telegraph*, is also disappointed that none of the expatriate astronomers who left Britain for clearer skies in the 1950s and 1960s was offered any of the chairs of astronomy which became vacant in Britain in the past couple of years.

The chairs which have been filled have been the two at the University of Cambridge, the Plumian Chair of Astronomy and Experimental Philosophy vacated by Sir Fred Hoyle and filled recently by Professor Martin Rees of the University of Sussex, and the Chair of Astrophysics vacated by Professor R. O. Redman which was filled by Professor Donald Lynden-Bell from the RGO. The other chair which has been filled most recently has been vacant twice during Margaret Bur-

FRENCH SCIENCE

Awaiting a Policy

from *La Recherche*

THE 1974 French science budget is one of recession, similar to that of 1973. The budget will allow research activities to be maintained at a constant level, but it is certain that very few new activities will be launched.

The *enveloppe recherche* for 1974 of Fr 4,056 million is 8.6 per cent more than it was the previous year. But an extra Fr 250 million has been put by for the L3S launcher which France has recently decided to construct. Because of the high rate of inflation in France last year, the increase barely covers present costs.

Basic research, however, gets more than the average increase, with the budget to be increased by 10.6 per cent. Those engaged in socio-economic research (transport, for example) will do even better with an 11.3 per cent increase in their budgets. Industrial research, on the other hand, does badly with a 5 per cent increase planned, but the government has decided to increase by 25 per cent the funds planned for improving the technological competitiveness of industry. But there is to be a cut-back in the funds for big nuclear programmes. Only 500 new jobs will

be created by the new budget, compared with the 700 which were created last year.

The inevitable part of the announcement of the plan by a representative of the Ministry of Industrial and Scientific Development was the acknowledgement that research no longer has as high a priority in France as it has been accustomed to in recent years.

The ministry had been forced to abandon the objectives of the sixth plan for research which covered the years 1971-75, and has substituted in its place a patchwork policy stressing different aspects every year. For example, aid to industrial research was sacrificed in 1973, but it is to be augmented substantially in 1974. It is clear that the Ministry of Industrial and Scientific Development, which exercises supervision over a great number of research bodies, such as the Atomic Energy Commission and the National Center for Space Studies, finds it increasingly difficult to define the broad lines of a science policy (or even of industrial policy). Unfortunately the minister, M. Charbonnel, has insufficient political weight in the present government compared with the Minister of Finance, M. Giscard d'Estaing, to argue persuasive enough a case for science to retain its priorities.

bidge's brief reign at the RGO. This is at the University of Sussex, where first Professor W. H. McCrea retired and was replaced by Professor Martin Rees; on Rees's appointment to the Plumian Chair, the job at Sussex was again vacant, and has now gone to Professor L. Mestel, previously of the University of Manchester.

Geoffrey Burbidge, speaking from San Diego this week, said that he totally supported his wife's actions. He said "we are both totally frustrated about the situation. The whole thing is a mess—we have tried and we have certainly failed". But there is one bright note. Margaret Burbidge, according to her husband, is willing to remain on the board of the Anglo-Australian telescope if she is asked to.

It was a year ago that Geoffrey Burbidge, in a letter to *Nature*, condemned British optical astronomy as being "third rate" (*Nature*, 239, 117; 1972). He said this week that he still stood by what he had written then. "The British astronomical establishment has consistently over the period since the war refused to face the real world or accept that anything was the matter, and when important decisions were made they were either hopelessly wrong or too late or both", said Professor Burbidge in 1972.

Is there a solution to the problems which Geoffrey Burbidge sees in British optical astronomy? In his letter critical of the way in which this branch of astronomy is organised he says that "the only hope [to retrieve the situation in Britain] would appear to be the creation of a new group or organisation with an international background which will set itself the single goal of creating somewhere in the world an optical observatory which, as far as climate and instrumentation are concerned, is second to none". It seems, from the experiences of the past two years, that at least this must be done to attract British expatriate astronomers back from the United States.

GLAXO LECTURE

Medical Research Policy

THERE is a need to bring young scientists more adequately into decision making, according to Professor W. S. Peart of St. Mary's Hospital, London. Professor Peart was speaking last week at the Royal Institution in a meeting organised by Glaxo to celebrate the twenty-fifth anniversary of the *Glaxo Volume*.

In his lecture entitled 'Medical Research is too important to be left to the researchers!', Professor Peart followed the life of an imaginary young man aiming towards a career in renal diseases but who was frustrated at every

turn by administrative changes. After endless disappointments the young man finally became a lobbyist for a scientific society and was under-employed ever after.

Professor Peart contrasted the career of today's young man with that of Richard Bright the early nineteenth century doctor whose brilliant career was helped along not a little by the apparent lack of any administrative ability to prevent him doing what he wanted.

Each of the obstacles the present-day Richard Bright encountered were tied in with specific actions by governments and learned societies, and lest it be thought that the system in the United States was any better, Professor Peart threw in trans-Atlantic swerves in policy for good measure. He enunciated three principles which seemed to permeate much policy making; direction without discussion, direction despite discussion and change for change's sake.

What remedies did he see for the problems of today? The educational system is a hurdle race with a very rigid set of rules governing the careers appropriate to particular qualifications. He doubted that there was as great a need for specialising as medical training implied. He contrasted the four or five academic hurdles that are placed in the way of the prospective researcher if he first qualifies in medicine, with the two that all other researchers need to overcome.

Scientists should watch very carefully the growth of a centralised governmental control for research linked to the Treasury. Once a central bureaucracy is installed it is difficult to keep decisions away from governmental influence and political control. Unprejudiced advice to governments was needed, and yet governments would willingly absorb advisory bodies to ensure that they got the advice they wanted. Professor Peart drew parallels here with President Nixon's cancer initiative and political attempts in the United States to change student training programmes and make students contract with the state to repay their education bill.

Scientists could do a much better job of communicating with society and in particular with politicians said Professor Peart. Several learned societies in the United States had made a start with the congressional science fellowship programme and although he was not convinced that this was anything more than a public relations exercise, Professor Peart felt there was a need for some structure in relations with politicians, particularly those outside the government. A science advisor as such finds himself very constricted by his allegiance to government and hence to

a reticence and a segregation from the main body of science.

Professor Peart concluded by emphasising the importance of a continuing commitment to the young. New ideas come from the young and all organisation in science and medicine should be directed towards helping the best young minds to be creative. This could effectively be done only if young scientists were given a greater perception of the decision-making process and a chance to get involved in that process themselves.

SCIENCE POLICY

Cambridge's Gain

"THE New Master of Jesus" ran the headline in last Saturday's *Guardian* over an article which announced that Sir Alan Cottrell, the British Government's Chief Scientific Advisor had been appointed master of a Cambridge College. A higher position than this would be hard to envisage for Sir Alan, but it will not be until next April that he will be leaving the corridors of power and committing himself to Jesus.

Sir Alan will succeed Sir Denys Page, the present master, who is retiring on March 31. Sir Alan is no stranger to Cambridge, for, as well as still living there, he was Goldsmiths Professor of Metallurgy at the university from 1958 to 1965.

After then he was at the Ministry of Defence, first as the Deputy Chief Scientific Advisor and in 1967 he was made Chief Advisor. In 1968 he moved to the Cabinet Office where he was Deputy Chief Scientific Advisor to the government before he was made Chief Advisor in 1971.

Sir Alan's move opens the way for a rearranging of the powers of the Chief Scientific Advisor if it is felt necessary. It has been apparent for some time that the reorganisation of government science in the wake of Lord Rothschild's report and the subsequent white paper in the summer of 1972 has given the departments much more power than they had previously. It can be inferred that the Chief Scientific Advisor's powers and influence have changed, but there are no outward signs yet of the extent of these changes.

Correction

IN last week's *Nature*, 245, 346, Professor Tinbergen was erroneously credited, in column 2, with early work involving "seal studies". This should have read "field studies".

Short Notes

Atlantic Observatory

TENERIFE has finally been ruled out as a site for the proposed British observatory in the Northern Hemisphere on political grounds. The current state of tension between Britain and Spain over Gibraltar has forced the government to advise the optical astronomy community of this fact.

Visibility tests are at present going on in other non-Spanish islands in the Atlantic, but it seems that it will be several months before these are completed. Once a site is decided on, the next decision will be whether or not the 98-inch Isaac Newton telescope at Herstmonceux should be moved to this site or whether a new telescope or telescopes should be built.

Dr Alan Hunter, the next Director of the Royal Greenwich Observatory, said this week that the cost of moving the telescope would be an "appreciable fraction of the cost of building a new one". Dr Hunter does not favour moving the telescope unless it proves impossible to raise the funds to build a new instrument.

The Science Research Council last year asked Professor Margaret Burbidge to investigate the costs and other benefits of moving the telescope. So far, the council has not received any official report.

Once a suitable site is found for the observatory the Science Research Council will be asked to finance the building of three telescopes, one of 1 metre, one of 2 metres and one of 4 metres. The total cost of the project has been estimated to be £15 million.

BA diversifies

THE British Association for the Advancement of Science is continuing its efforts to become more relevant. As a prelude to the association's annual meeting to be held at the University of Stirling next year a series of meetings will take place which will discuss the influence of science on the life of the nation. The first will be in November.

The British Association has often been accused of concentrating all its efforts into producing an annual meeting but doing little else for the remainder of the year. This is in the process of being remedied, particularly with the recent appointment of Dr Magnus Pyke as both chairman of the council and as secretary of the association. Dr Pyke is most keen for the association to diversify its activities.

The first of these meetings will take place at the University of Dundee, when the impact of the current upsurge in geological sciences on society will be discussed. Perhaps the most relevant

session at this meeting will be that in which the competitive position of coal with respect to oil and gas will be discussed. Mr A. M. Clarke the Deputy-Chief Geologist with the Coal Board will take place in the discussion as well as an economist, Professor C. Blake and a Civil Engineer, Professor A. R. Cusens, both from the University of Dundee.

Shining Brightly

Two announcements last week confirm that the 154-inch telescope to be built at Siding Spring in Australia is both on schedule and that it will be the most accurate telescope in the world when built.

Reyrolle Parsons has completed the three and a half year process of grinding and polishing the primary mirror for the telescope and it is now on its way to New South Wales where it will be installed at the end of this year.

The Department of Trade and Industry in a back slapping announcement said that three engineers at the National Engineering Laboratory at Glasgow had developed a new calibration technique which will ensure that the 20 tons of telescope, which will have to be rotated, will be capable of "more precise star tracking than any other major astronomical telescope in the world".

The techniques developed by both Reyrolle Parsons and the National Engineering Laboratory might again prove useful to astronomers. If funds are made available to build a British observatory on some Atlantic island, then the exercises will not be very expensive one-off operations, for the plans are, as we report elsewhere on this page, to build three telescopes in the proposed observatory.

Pasteur Problems

THE Pasteur Institute in France has been shaken recently by a serious financial crisis. Professor Jaques Monod, the director, has announced that the institute has been forced to close several research units and thirty scientists and a hundred technicians have been dismissed.

The announcement has, not unexpectedly, caused an uproar in France, for in addition to this announcement, Professor Monod revealed that the institute had requested financial support from the government. This State support would go towards financing the services which the institute provides to the community in public health and, for financing immunology and microbiology.

Directors of the institute are, however, keen to maintain the independence

of the foundation and so only partial help from the government has been asked. The hope is that more money can be earned in the next few years by increasing the turnover obtained from the institute's production services.

Curien for Aigrain

PROFESSOR Pierre Aigrain has been replaced as Deputy-General for Scientific Research in France by Dr H. Curien who was until recently Director of the National Centre for Scientific Research (CNRS). As reported recently in *Nature* (244, 64; 1973) Professor Aigrain, after a five year term as advisor to the French Government, took up a teaching position at the Massachusetts Institute of Technology.

Dr P. Gregory has replaced Dr Curien at the head of CNRS.

Whiter than white

TAKEN at face value, the statistics published in the Procter and Gamble annual report on the use of soap per person in Britain, can be taken to imply that the Briton of today is almost five times cleaner than his great great grandparents were in the early 1840s.

At that time each person used on average 6½ pounds of soap a year. In 1871 this had risen to 10½ pounds, while in 1911 each Briton was disposing of 18 pounds every year. Today 29 pounds a year of soap are used per person for all domestic and industrial purposes.

Surprisingly the increase in use of soap is almost linear and not exponential. Has the soap industry been living in an unreal world for the past hundred years?

Northern Lights

LAST week an experimental campaign started to investigate the auroral sub-storm phenomena at various stages of development and altitude.

The plan is to launch five Skylark rockets from Norway's Andoya rocket range. The first of these was launched last week and the other four will be launched in conjunction with ten Aerojet Petrel rockets.

Nineteen experiments are to be carried aboard the Skylarks and twelve on the Petrels. The chief objective will be to examine the magnetic sub-storms which occur near the Arctic Circle.

The research is sponsored by the Science Research Council and is managed by the Radio and Space Research Station. The experiments have been prepared by groups from the Universities of Birmingham, Sheffield, Southampton and Sussex, University College, London, the Radio and Space Research Station and the Royal Institute of Technology in Sweden.

NEW WORLD

Public Interest Groups Meet

by our Washington Correspondent

PUBLIC interest groups have, during the past few years, gradually established themselves as an important feature on the political landscape in the United States. But although they have won some stunning victories, they often find it difficult to match the huge scientific resources that large corporations and government agencies are able to call upon. Moreover, the haphazard growth of the public interest movement has resulted in much fragmentation and little coordination.

Some of these problems could, however, be alleviated following a meeting held in Washington recently. Leaders of many public interest groups met to discuss the possibility of setting up an organisation to coordinate the groups' activities, provide a registry of scientists who are willing to lend their expertise on specific problems, and generally provide a service to existing organisations.

The idea is chiefly the brainchild of Dr Samuel S. Epstein, Professor of Environmental Health and Human Ecology at Case Western Reserve University, who has played a leading role in the fight against unsafe food additives, drugs and pesticides. Epstein persuaded the Monsour Medical Foundation, a Pennsylvania-based medical organisation, to sponsor the meeting, and the foundation will also underwrite some of the costs of getting the new public interest organisation off the ground. It could greatly increase the effectiveness of public interest groups in the scientific arena, but a few doubts were raised at the meeting about the need for yet another outfit.

Epstein is quick to point out, however, that the new organisation will not in any way compete with existing groups, but will complement their activities. The plan is for it to avoid such causes as bringing court suits, and testifying before Congressional committees, but to help other public interest groups in their efforts in those areas. First, it will assist in the publication of a newsletter, to inform each group of what the others are doing, and it will also probably help to set up *ad hoc* coalitions to handle specific issues. Second, and perhaps most important, it is hoped that it will be able to establish numerous contacts within the scientific community so that groups can be put in touch with expert help on the matters they are fighting. It will also probably be an information clearing house, and there is even talk of starting up a

journal devoted to scientific issues involving public concerns.

Epstein said at the meeting last week that the public interest movement has been forced to take a "brush fire" rather than an anticipatory approach—unsafe pesticides and drugs have been banned, but only after they have been used extensively, and there is always a new fight imminent—and he said "the odds are against us if we continue with this approach".

It is ironic, however, that just as the public interest groups are considering

setting up this new coordinating body, some of the most powerful organisations are in danger of losing an important source of funds. The *Washington Post* recently disclosed that the Ford Foundation is considering withdrawing its financial support from legal groups which have been active in environmental concerns. The reason, it seems, is that the foundation would like to focus more on environmental research than on legal action, and has asked the groups to investigate other sources of funding by 1975.

SAKHAROV

Keldysh Replies

by our Washington Correspondent

MYSTISLAV KELDYSH, President of the Soviet Academy of Sciences, last week publicly defended the campaign by Soviet authorities against Andrei D. Sakharov, the Soviet physicist who has been in trouble for his outspoken criticisms of Soviet society. The defence came in an unusual open letter sent to Dr Philip Handler, President of the United States National Academy of Sciences (NAS), in which Keldysh said in effect that Sakharov has brought the attacks on himself and that the NAS should stop trying to meddle in the internal affairs of the Soviet Union. The letter was an official reply to a telegram Handler sent to Keldysh last month, protesting the harassment Sakharov has received in the past few months.

Handler warned, in his telegram last month, that further harassment or detention of Sakharov would seriously jeopardise the scientific agreements that have recently been reached between the United States and the Soviet Union. He was particularly critical of a letter attacking Sakharov which was published in *Pravda* and signed by some forty members of the Soviet Academy of Sciences. Such attacks, he said, "revive memories of the failure of our own scientific community to protect the late J. R. Oppenheimer from political attack".

But Keldysh replied that "My colleagues from the Praesidium of the USSR Academy of Sciences and I cannot but strongly object to the content and tone [of Handler's telegram], which are not in accord with either the substance of the matter or with the spirit of the relations between our Academies".

He pointed out that the *Pravda* letter condemned Sakharov for urging western governments to insist on changes in internal Soviet policies before accepting normalised relations with the USSR. "By such acts", Keldysh said, "Sakharov harms the interests of not only the people of the Soviet Union, but also of other countries who sincerely aspire for a detente".

Keldysh's letter goes on to point out that no action has been taken against Sakharov and that "even today he has every opportunity actively to conduct research". That, at least, seems to imply that some basic freedoms have not yet been denied to Sakharov and that he may be safe from further harassment, at least for the time being.

One of the chief points in Handler's telegram was that the treatment of Sakharov could upset the scientific accord and exchanges between the United States and the Soviet Union, to which Keldysh replies: "The attempts to use this exchange to influence the Soviet scientist's political stand are absolutely unpromising, let alone the ethical aspects of this matter". He concluded that "We stand for an extensive development of scientific cooperation provided that the traditions and way of life of every country are mutually respected and observed".

An important aspect of Keldysh's letter is that it was published in the Soviet weekly newspaper *Literary Gazette*. It was therefore written with at least an eye on internal propaganda, and its seemingly uncompromising stand against Sakharov should thus be seen in that light. Since the letter contains a summary of Handler's telegram, its publication also brings to the attention of the Soviet public the fact that the harassment of Sakharov has sparked off a series of protests in the West.

CANADA

Science Policy in Canada

from a Correspondent

DR ALEXANDER KING of OECD, regarded by some as the father of National Science Policy Studies, has commented on the Canadian effort in that field, saying that "In no country has the legislative branch undertaken such a thorough and detailed job." The recently released third and final volume completes the 837-page report of the Senate Special Committee on Science Policy with sweeping recommendations for restructuring government science. Since the Committee demonstrated in earlier volumes that basic research is dominated by in-house work of government laboratories, their restructuring has wide implications for all science in Canada.

The Committee examines four possible approaches that can be used to determine overall organisation for science policy: pluralism, coordination, integration and concerted action. From the experience of other countries, the Committee comes out heavily in favour of the concerted action model for which it finds an example in France. Convinced that coordination by discussion and advice does not work without either a stick or a carrot, the Committee recommends that the Ministry of State for Science and Technology should become a super science ministry responsible for vetting the programmes and submissions of individual departments and then justifying the whole science budget to the Treasury Board.

With the new, expanded role for the ministry would come a reorganisation of the Science Council. This lively, erudite and well informed body was established in 1966 to advise the government on science policy. It seems that its advice has not made much impact. Following the proposed reorganisation, the council's mandate would be extended to cover the social sciences and humanities; it would establish closer links with the Economic Council and there would be a number of changes in the membership. If the government does not form a super science ministry then the Science Council should be abolished. "There is no need for two frustrated advisory bodies."

The report stresses that there is no continuum from basic to applied research to innovation. It is admitted that most Canadians do not agree with this point. The reason for stressing it is to support the Committee's prescription for reorganisation of federal activities with basic research at the National Research Council (NRC); applied institutional research at Industry, Trade and Commerce, and other mission-orientated research in departments which are also responsible for policy formulation. The

NRC would be thus transformed into an institution for intramural basic research in the physical, life and social sciences. It would do "university-style" work, partly on a fee basis, for government agencies and private companies. Its industrial research would be transferred to a proposed Canadian Industrial Laboratories Corporation. According to the report, NRC would abandon its "disguise" or "false image" of performing a practical mission. Many, who feared a total abolition of NRC after the publication of earlier volumes of this report, can now breathe a sigh of relief.

The report includes a number of other recommendations:

- Special status and federal financial support for the Royal Society of Canada and the Association of the Scientific, Engineering and Technological Community as the two major representatives of scientists and engineers in Canada;
- A federal-provincial committee on science and technology;
- A Crown corporation to develop more adequate French terminology in science and technology;
- International scientific missions to keep the government informed on science policy and technological advances in other countries; and
- A series of awards to be given to Canadian industry for significant inventions and technological innovation.

When all is said and done and 837 pages of report have been put to rest, one is left with two feelings: respect and even admiration for the breadth and thoroughness of the Senate Committee's investigation; and a question as to whether the effort was worth it at all. It is just possible that the internal mechanisms of self regulation, peer review, internal consistency and mutual respect are the best possible methods for directing the science towards the socially desirable goals. With a 300-year tradition, science just might find the best possible direction if left alone.

ENERGY

Conserving Energy

by our Correspondent

WHILE the United States Federal Government is struggling to meet the long term challenge posed by the energy crisis, the American Association for the Advancement of Science has been organising a series of workshops to discuss what can be done in the short term by individual states. The first of these, held in Chicago on October 11 to illuminate the problem in Illinois, cast into relief a combination of technological sophistication and total absence of political or practical common sense which has more than parochial implications.

This was nowhere clearer than in the

first session, which was concerned with building design and management. New York architect Richard Stein used as an example of false economy a large building in Michigan in which the lights are left on for twenty-four hours a day, to prolong the life of the bulbs. The irony of this policy is that although twenty-four hours of continuous switching on and off would undoubtedly use up the bulbs faster than twenty-four hours of continuous illumination, a pattern of ten hours of light and fourteen hours of dark gives an average life to the bulbs of four years, but bulbs in continuous use fail after an average of eighteen months.

Both the lighting and the ventilation standards laid down by the building codes are in any case, according to Stein, well in excess of physiological needs. He compared the IBM recommendation of 70 foot candles of illumination (apparently set by the somewhat archaic criterion that the fifth carbon copy be as legible as the original) with British classroom standards of 10 foot candles. The British, Stein claimed, discovered that to raise reading efficiency from 90 to 95%, it would be necessary to raise the light level by a factor of ten which did not seem worth the foot candles.

Somewhat more radical proposals for energy conservation in buildings were tackled by Gerald S. Leighton, of the United States Department of Housing and Urban Development. One obvious need now apparently being met is that for a detailed economic analysis of the total energy system—a method of storing waste heat for re-use in supplying energy to buildings.

Industry, Leighton believes, should resign itself to the use of high-sulphur coal—an energy source that is not in short supply. The Clean Air Act has made such a solution impractical for the individual home owner; but industry can afford to develop and install the "clean-up technology" to cope.

The kind of measures proposed by industrialist Gerald S. Bataille and economist George S. Tolley of the University of Chicago, were unappealing, although probably realistic. Industry could best be induced to conserve energy, Bataille asserted, if the savings could be expressed in dollars. Tolley applied the same principle to the private individual, with a simple equation. His investigations have led him to conclude that if fuel were made 10% more expensive, the individual would use 10% less of it. The increase in the use of electricity, he believes, is a simple function of the reduction in its cost over the past decades. There is no reason to believe that consumption will continue to increase at the same rate; and there is a simple way of stopping it.

NEWS AND VIEWS

Radiation-induced Mutation in Man

MUCH effort, ingenuity and paper, in addition to flies, mice and mammalian cells in culture, have been expended in attempting to answer the important practical question of how best can the genetic risks to man of exposure to ionising radiations be estimated. Human genetic disease is a consequence of subtle changes at the gene level, or of alterations in chromosome structure or number which are detectable by the cytogeneticist. These kinds of alterations can be induced in human somatic cells exposed to radiations and there is much cytogenetic information on the radiation response of human chromosomes in blood cells, from irradiated patients or cultures, and a good start has been made on measuring radiation-induced mutation frequencies at a number of loci in rodent and human cultured cell lines. Although genetic damage in somatic cells is important to the individual, insofar as future populations are concerned it is of less consequence than damage transmitted to offspring in the germ cells. There is, however, an almost complete absence of information on radiation-induced mutation in the germ cells of man, so that in order to quantify risk in terms of mutation rate per locus per rad, it is necessary to extrapolate from experimental results obtained with other organisms.

In 1956, government committees in the United States (the BEAR Committee) and the United Kingdom (the Medical Research Council committee) relied largely on extrapolating from data obtained from irradiated fruit flies and the then emerging data from the so-called megamouse experiments undertaken at Harwell and Oak Ridge. Later government appraisals, and including those by the United Nations Scientific Committee on the Effects of Atomic Radiations (UNSCEAR), have drawn heavily on the wealth of data that has been accumulating on the radiation response of mouse genes and chromosomes. It may seem a big jump to extrapolate from observed radiation-induced mutation rates in mouse germ cells to predicted mutation rates in human germ cells, but this is the nearest comparison that can be made.

Confidence in the mouse-man extrapolation would be bolstered if it could be shown that a close similarity existed between the radiation-induced somatic genetic damage in these species. Although there is a plethora of cytogenetic information on radiation-induced chromosome damage in human blood leukocytes, it is only recently that attempts have been made to obtain equivalent information on mouse cells. This type of information helps to form a 'quantitative bridge' between mouse and man and recent data of this type would suggest that man may be equally as sensitive as the mouse to the production of specific-locus mutations which are the result of minute chromosome deletions (Brewen *et al.*, *Mutation Res.*, **17**, 245; 1973). A somewhat different approach to the problem of extrapolation is described on page 460 of this issue of *Nature*, where Abrahamson, Bender, Conger and Wolff report on a remarkable relationship between mutation rates and DNA content in a wide variety of organisms, and suggest that these data may be used with confidence to extrapolate from mutation rates obtained in experimental organisms to man.

Within a species, different genes may show a wide difference in both their spontaneous mutation frequency and in the frequency with which they will mutate after exposure to radiation. For example, in the case of a number of well-studied loci in the mouse, there may be a thirty or forty-fold difference in the rate of radiation-induced mutability in different genes. Mutability is also influenced by a variety of factors, including, for example, intensity of radiation, cell stage treated, but these differences are relatively minor perturbations when considered in relation to the 1,000-fold difference that is observed in the response between different species.

What Abrahamson *et al.* have now discovered is that, when forward mutation rates per locus per rad are normalised to the amount of DNA per cell nucleus, then the 1,000-fold difference between species virtually disappears, and the mutation rates in bacteria, fungi, insects, higher plants and mammals are all essentially the same. Thus, radiation-induced mutation rates per locus are directly and simply related to the DNA content of the genome—a surprising finding. This remarkable association between DNA content and mutation rate now allows a further bridge to be utilised in extrapolating data from radiation-induced mutation rates in lower organisms to man. The strength of this bridge is already indicated by the fact that the mutation rates for man which may be predicted from the graph presented by Abrahamson *et al.* accord fairly closely with the induced mutation rate already obtained in mammalian (including man) somatic cells in tissue culture.

Why the induced mutation rate per locus should be dependent on the total DNA content of the nucleus presents something of a puzzle. Target theory, with DNA as the target, would suggest that if structural genes are the same size in bacteria as in man, then the mutation rate per locus would be independent of total DNA and equivalent between diverse species for genes coding for similar proteins. The authors point out that since this is not the case then either the nucleus, and not the locus, determines target size, or the size of the locus is proportional to total genome size for the species. This last proposal is compatible with previously proffered models in which the functional genetic unit in eukaryotes may contain not only the structural gene but also the controlling elements, thus increasing the size of the mutational unit. H. J. E.

No Trip-up This Time

THAT transcription starts at the operator region—for the *lac* operator, it is a small segment of double-stranded DNA, twenty-seven bases long—is no longer in doubt (see *Nature*, **245**, 183; 1973). When an active repressor protein is bound to the operator DNA, no transcription occurs since the promoter region to which RNA polymerase binds is generally located distal to the first (or only structural gene, so a blockade is created for any movement of RNA polymerase. When the repressor is removed, the RNA polymerase can then pass over the operator region and begin transcription. The lactose

catabolic system is the best studied example of negative control. Here the active repressor protein (product of the *i* gene in *Escherichia coli*) will always bind an intact operator region unless an inducer is present. The inducer, probably a β -1-6 galactoside (note that lactose is not, but is a β -1-4 galactoside), elicits the synthesis of β -galactosidase by inactivating the repressor protein. Not all catabolic operons act like lactose protein, however, and positive control can also exist.

Sun and Weather

THE possible and probable effects of variations in solar activity on meteorological phenomena on Earth have long provided a talking point. Over the years, there have been considerable changes in the climate of opinion in which these relations are discussed, ranging from enthusiasm to downright scepticism. Perhaps partly because of this, but no doubt also in large measure because the subject is truly interdisciplinary and attracts interest from astronomers, meteorologists, earth scientists, electrical engineers and others, every few years there seems to be a general rediscovery of the relationship between solar activity and meteorological processes. What seems to be the largest and most general such rediscovery is now in progress; this time, because of the improvement in monitoring of both solar activity and meteorological phenomena over the past twenty years, there is a real chance that the subject will gain permanent respectability.

In an article on page 443 of this issue of *Nature*, King suggests that one mechanism for a link between solar activity and the weather might come from the injection of energetic solar particles into the atmosphere over the north Atlantic; changes in solar radiation during the sunspot cycle seem to be accompanied by secular meteorological changes that affect temperature and pressure in the northern hemisphere. Work of this kind has obvious importance to weather prediction.

The broad interest in the subject in general, and the seriousness with which this kind of work is now regarded, is highlighted by the news that on November 7 and 8 there will be a symposium on "Possible Relationships Between Solar Activity and Meteorological Phenomena", sponsored by NASA/Goddard Space Flight Center and held at the centre in Greenbelt, Maryland. This will bring together "astronomers, meteorologists, geophysicists and other interested scientists"; on the European side of the Atlantic, a similar symposium is being planned by the Radio and Space Research Station, Slough, in collaboration with the Climatic Research Unit which is based at the University of East Anglia. The symposium will be held at RSRS late this year or early in 1974, and is being organised by King. These two symposia seem likely to be remembered as landmarks in the development of a useful understanding of the processes by which changes in the Sun's activity affect meteorological phenomena. J. G.

Biosynthetically too, it is possible to envisage either a negative or a positive control scheme. The former has been, by far, the most popular and for a while it seemed that positive control may be hard to demonstrate. It seems quite likely, however, that positive control may exist for the histidine and the isoleucine valine schemes.

Conventionally, repression in a biosynthetic pathway can be assessed by adding a potential source of the repressing metabolite(s) to the growth medium and then measuring enzymatic activity. The ideal system would be one to which the actual repressing metabolite (=corepressor) can be added, as seems to be the case in tryptophan biosynthesis (Yanofsky and Mosteller, *J. Bact.*, **105**, 268; 1971). Thus the repressor is synthesised in an inactive form (=aporepressor) and will only bind the operator region when activated. In the case of tryptophan biosynthesis five structural genes comprise an operon, with a contiguous operator region and a regulator gene, *trpR*, located far distant. The product of the *trpR* locus, the putative aporepressor *in vivo* of tryptophan biosynthesis, has been purified fifty-five times by Zubay *et al.* (*Proc. natn. Acad. Sci., U.S.A.*, **69**, 1100; 1972). *In vivo* repression of the pentagon of structural genes in the operon requires the *trpR* protein product and tryptophan (Morse and Yanofsky, *J. molec. Biol.*, **44**, 185; 1969). Ten years earlier a similar observation was made by Cohen and Jacob (*C.r. hebd. Séanc. Acad. Sci., Paris*, **248**, 349; 1959).

The speculation was all brushed aside by the appearance of accompanying papers by Squires *et al.* and Rose *et al.* from Yanofsky's group (*Nature New Biology*, **245**, 131 and 133; 1973), and a brief report by McGeoch *et al.* in the same issue (page 137). The partially purified protein, product of *trpR*, together with tryptophan was added to Zubay's *in vitro* transcription system and transcription was stopped. Moreover, only when a functional operator is present (the *trpO*⁺; operator⁺) does this repression occur; templates which carry a mutant operator, for example, *trpO*⁻ (operator-constitutive), show no repression.

By way of summary the following points can be made about these interesting papers:

- As mentioned earlier, *trp* operon transcription is specially blocked when the partially purified *trp* repressor is added to the *in vitro* transcription system.

- Initiation of transcription on the *trp* operon *in vitro* occurs at, or very near, the *in vivo* site of transcription initiation.

- Regulation seen *in vitro* precisely mimics that which occurs *in vivo*. The addition of tryptophan to the growth medium causes repression and moreover tryptophan analogues that cause *in vivo* repression also repress *in vitro*; those non-repressing tryptophan analogues do not do so either *in vivo* or *in vitro*.

- As already mentioned, altered *trp* operons, with regulatory abnormalities *in vivo*, behave predictably when tested as templates *in vitro*. Thus *O*⁺ or *O*⁻ mutants are not repressible, and mutant templates that produce more *trp* message *in vivo* do the same *in vitro*.

- Three lines of evidence tend to rule out any involvement of tryptophanyl-tRNA synthetase (henceforth called 'synthetase') in *in vitro* repression: (1) When the *trp* repressor is further purified all detectable synthetase activity is separated from repressor activity; (2) a very appropriate synthetase mutant, showing a much reduced

affinity for tryptophan in the activating reaction, contains repressor with normal affinity for tryptophan in repression; and (3) a negative correlation exists between tryptophan analogue affinity for synthetase, and affinity for repressor.

• The lack of any detectable tRNA^{Trp} in *trp* repressor preparations, together with the negative correlation between charging of tryptophan analogues and their ability to repress, strongly suggests that tRNA^{Trp} is not required for *in vitro* repression.

No *trp*-up is likely but the temptation to pose an obvious question is too great to let slip by. Is *trp* unique? Probably not, as Maas (New York University School of Medicine) finds methionine and arginine repression without the involvement of their respective tRNA-amino acyl complexes; only the amino acid is required *in vivo* and *in vitro*.

From a Correspondent

PESTICIDES

Social Values

from a Correspondent

ECONOMIC and social values in the assessment of crop protection and pest control methods were considered at an international conference, organised by the Pesticides Group of the Society of Chemical Industry and held from September 7 to 11 at the University of Stirling.

C. Potter (formerly Head, Insecticide and Fungicide Department, Rothamsted Experimental Station) examined critically the effects of pesticides in improving health in man, in relieving hunger and on the environment. The world-wide programme, sponsored by the World Health Organisation, to eliminate malaria now protects 1,329 million people out of 1,802 million living in former malarial areas and control, mainly by insecticides, of the mosquito vectors of the protozoa causing the disease has reduced the world annual death rate from malaria from 6 million in 1939 to 2.5 million now. He quoted many such examples of disease control and pointed to the sad situation in Ceylon where, after reducing the incidence of malaria from 2.8 million cases in 1946 to 110 in 1961, the numbers had shot up to 2.5 million when spraying was stopped for economic reasons.

Both Potter and H. R. Fell, a farmer, stressed the magnitude of changes in agriculture in Great Britain since 1939; assisted by mechanisation, crop protection chemicals and fertilisers, food production has been doubled though there are a million less acres under cultivation

and 500,000 fewer workers on the land. Comparable changes have yet to reach the developing countries. Potter contrasted the performances in rice production of Japan, now an exporter, and India: average yields 5.1 and 1.6 tonnes per hectare and losses to pests and diseases 5% and 46%, respectively.

The evolution and functioning of the United Kingdom's Pesticide Safety Precautions Scheme were outlined by M. Cohen (Plant Pathology Laboratory, Harpenden) and A. Wilson (University of Liverpool), who is chairman of the Advisory Committee on Pesticides and Other Toxic Chemicals. Wilson stressed that, however carefully assessment is made and incorporated into recommendations, safety depends on the care with which pesticide products are handled and applied; he pleaded for more training of operators and communication with the public about what is going on. The need for greater publicity of the steps taken by pesticide manufacturers to establish the safety of their products was frequently echoed in the discussion. There was unanimous agreement with the praise given by K. Mellanby (Nature Conservancy, Monks Wood) of the working of the voluntary scheme in Britain, and he pointed out that through it restrictions were introduced on organochlorine seed dressings before the publication of Rachel Carson's *Silent Spring*.

There is a danger that the complete ban introduced for political reasons by several developed countries on some organochlorine insecticides may be extended, again for political reasons, to the developing countries before the latter have suitable replacements. P. O. Park (FAO, Chad) stated that the Food and Agriculture Organisation encourages substitutes to DDT whenever possible but it is unwilling at present to see the complete disappearance of the organochlorine insecticides, which are so economically valuable to the developing countries.

Several speakers expressed concern over increasing pressures on pesticide manufacturers by the regulatory authorities. The necessity for more stringent tests to ensure the safety of new pesticides and the smaller chances of discovering compounds with more selective action mean that development costs of a new pesticide have doubled in the past 5 years and the time interval from discovering to marketing a product has increased from 5 to about 7 years, with a corresponding decrease in the time remaining for protection under the firm's patent. If the new compounds needed to combat the growing threat of resistance in pests and fungi are to be obtained, then pesticide prices must rise and ultimately the cost will have to be borne by the consumer. Greater uniformity between countries in the require-

ments to establish the safety for a particular use was suggested as one way of reducing the development costs of a new pesticide.

Participants from overseas stressed the need for uniformity in the residue tolerances permitted in different countries for a given pesticide/crop combination and in the methods used for determining them if international trade is not to be hindered. Some countries, for example the United Kingdom, do not have limits set by legislation but depend on codes of practice and a stated minimum time interval between the last application and harvest. P. Slade (Imperial Chemical Industries Ltd) referred to the Commission of the Pesticides Section of the International Union of Pure and Applied Chemistry which, at the request of FAO and WHO, is examining residue methods for their suitability for international regulatory application. The setting of tolerances based on sound agricultural practice for a given crop in widely different climatic conditions would involve considerable negotiation.

The value of pest and disease forecasts, which reduce the need for prophylactic treatments and permit optimum timing of pesticide applications, was stressed by several speakers and Cohen called for more work to establish the levels at which pest and disease damage to crops becomes economically significant. Problems also change with time: R. Hull (Broom's Barn Experimental Station) explained that modern herbicides allow sugar beet to be sown as pelleted seed at a low density, making it more important to protect the young plants from soil pests. The value of growing plant varieties resistant to pests and diseases was noted by several contributors and the dangers inherent in monoculture emphasised. Currently Golden Promise, a barley variety excellent for malting but highly susceptible to mildew, constitutes 65% of the 430,000 acres of the spring barley crop in south-east Scotland. It provides a residue of inoculum and potentially a very serious situation should strains of the pathogen tolerant to modern fungicides be selected.

VIRUSES

SV40 Defectiveness

from a Correspondent

It has been known for some time that when SV40 virus is serially passaged, undiluted, in BSC-1 monkey cells, virions are produced which contain circular DNA molecules consisting of host and viral sequences (Lavi and Winocour, *J. Virol.*, **9**, 309; 1972). This suggests that recombination between host and viral DNA occurs readily during lytic infection in these particular circumstances. If the viral DNA is

integrated into the host chromosome, recombination could involve integration followed by inaccurate excision, and there could then be secondary recombinations between the hybrid viruses so formed and host sequences in subsequent passages. However derived, the defective SV40 virions are a potentially useful source not only of specific deletions of viral DNA, but also of specific pieces of host DNA, which might simplify studies of mammalian, as well as viral, gene function.

In order to understand exactly what has happened during recombination between viral and host molecules, a method of dissection of defective DNA is required, followed by subsequent identification of its components. The bacterial restriction endonucleases now provide a tool for this, because the cleavage sites in normal SV40 molecules and resulting DNA fragments are characterised (Danna, Sack and Nathans, *J. molec. Biol.*, **78**, 363; 1973). Deletions can be detected by electrophoretic analysis of fragments and this technique, combined with DNA:DNA hybridisation experiments, would enable an assessment to be made of the nature and extent of deletions and substitutions in the viral DNA.

Considerable progress along these lines is now reported by Brockman, Lee and Nathans (*Virology*, **54**, 384; 1973). They have used the restriction enzyme of *Haemophilus influenzae*, which breaks SV40 DNA into eleven fragments, and have analysed the fragments produced from defective DNA after up to twenty serial, undiluted, passages in BSC-1 cells. The results show that after four passages widespread deletions have been produced in the SV40 DNA. This is reflected, first, in the fact that the DNA molecules recovered are shorter; and, second, in the appearance of new restriction fragments distinguished by their electrophoretic mobilities. All the original eleven fragments are still present, but not in the same amounts—fragments D, E, F, G, J and K are slightly decreased. The authors have ruled out the presence of whole parental DNA molecules in the defective virion population, which could be the sole source of the eleven normal restriction fragments.

After eight passages, many new restriction fragments can be detected. The decreased amount of fragments D, E, F, G and K is much more noticeable. Interestingly, these fragments are contiguous in the restriction map of the SV40 genome (Danna *et al.*, 1973) and make up the region transcribed late in lytic infection (Khouri, Martin, Lee, Danna and Nathans, *J. molec. Biol.*, **78**, 377; 1973). By twelve passages, the fragment pattern is much simplified and remains so after further passages. There are five principal fragments after twenty passages, none of which has the

same electrophoretic mobility as the original fragments.

Hybridisation analysis of defective DNA showed that up to the fourth passage, the sequences present are almost entirely derived from SV40. From the eighth passage on, a decreasing percentage of the defective sequences anneal to SV40 DNA. Hybridisation to BSC-1 DNA revealed the presence of host sequences, but the conditions used in the experiments suggested that these were mostly non-repetitive sequences.

The conclusions that may be drawn from this interim analysis are that deletions occur early in a sequence of passages, but that they are not initially accompanied by substitutions with host DNA. Second, the deletions probably occur at many sites, but apparently more frequently in the area of the genome including the sequences for 'late' viral functions. Third, as the number of passages increases, new dominant species of defective molecules are produced, in which less than one third of the sequences are derived from SV40. Brockman *et al.* suggest that since the defective DNA molecules presumably replicate faster than normal, those that retain their replication functions, or which in some way enhance them, will be at a selective advantage. This may explain why fragments D, E, F, G, K and J are affected preferentially, whereas fragments A, B, C, H and I, which include the site of initiation of DNA replication, are retained. It is also possible that the host sequences which become part of the hybrid defective molecules confer some advantage in replication.

ENTOMOLOGY

Insect Flight

from our Insect Physiology Correspondent
THE biennial symposia of the Royal Entomological Society of London, which are condensed into a day and a half, are planned to provide accounts of the current state of knowledge in a form adapted to a non-specialist audience. The specialists who give the talks are therefore presented with a formidable task. The symposium on insect flight held on September 20–21, which was organised by Dr R. C. Rainey, dealt with two principal topics: the mechanics of insect flight, and the flying insect in its environment.

J. W. S. Pringle (University of Oxford) gave a lucid account of the two types of flight muscles: synchronous and asynchronous in regard to their relation to nervous stimulation. He then discussed the mechanical sense organs which inform the insect about its own wing movements; and the wing mechanism as it exists in the honeybee.

B. Mulloney (University of California,

San Diego) dealt with the antithesis between centrally generated motor patterns of flight control and the function of sensory feedback; and then with the light that has recently been shed on this problem by intracellular recordings from motor neurones and interneurones in the locust. W. Nachtigall (Universität des Saarlandes) reviewed his extensive work on flight mechanics and aerodynamics in the cockchafer *Melolontha*, in blowflies and in the honeybee; and also on the gliding flight of butterflies. Kinematic parameters and power output are tolerably well understood in these insects.

T. Weis-Fogh (University of Cambridge) reviewed the steady-state aerodynamics which has been shown to account for the normal flight of the locust and other insects, and then gave a brief account of his work on novel types of non-steady aerodynamics dependent on wing twisting, as utilised in minute insects at low Reynolds number and also in hovering insects and in many butterflies.

R. C. Rainey (Centre for Overseas Pest Research, London) surveyed the behaviour of flying insects in relation to atmospheric circulation. This involved consideration of long-range migration and global wind systems, the biological cohesion of flying locust swarms, convergence of insects by wind systems and the bearing of this on crop damage and the formation of spray targets. E. Betts (Centre for Overseas Pest Research) related these matters to the problem of practical forecasting for infestations of such tropical migrant pests as the desert locust and the African army worm. R. J. V. Joyce (Cranfield College of Aeronautics) discussed the possibility of utilising knowledge of aerial transportation, dispersal and concentration in order to control migrant insects by direct attack from the air. The methods and the detailed behavioural results obtainable in radar recordings of flying insects were then described by G. W. Shaefer (Loughborough University of Technology), who related such findings to the study of insect populations and the influence of wind fields on flight.

M. Lindauer (Universität Würzburg) gave a general survey of orientation in the honeybee during foraging and homing flights. This involved considering the sensory stimuli used in identifying the goal, and the methods of localisation—including measurement of distance, orientation by the Sun with the necessary corrections for time, wind and so forth, and the influence of the Earth's magnetic field. C. G. Johnson (Coconut Industry Board, Jamaica) stressed the lability of the flight system, the fact that flight activity changes during adult life—largely in relation with reproduction, which is commonly preceded by migration or dispersal.

These life cycles are tied in with metabolic changes, muscle autolysis, and so forth. R. Wootton (University of Exeter) dealt with the fossil record and insect flight. No insect fossils exist from the Devonian when winged insects are believed to have appeared, but from consideration of the earliest fossil forms he supported a monophyletic origin of insect flight by way of a gliding phase in Devonian insects with thoracic paranal lobes. V. B. Wigglesworth (University of Cambridge), after reviewing theories of the origin of wings, supported the gill theory and suggested that wings evolved from the styli of the thoracic coxae in a secondarily aquatic apterygote, perhaps of thysanuran type; and he restated his 'dispersal theory' of the origin of flight on this basis.

LEPROSY

New Treatment?

from a Correspondent

ONE of the many enigmas of leprosy concerns the particular predilection of the causative bacillus for human nerve tissue, a site which no other infective bacilli are found to occupy. The disease has received little attention from research workers, so that the nature of the causative organism remains unknown.

Since 1967 Prabhakaran's group have been concerned with the presence and implications of an unusual tyrosinase in *Mycobacterium leprae*, and in the current *Leprosy Review* (44, 112; 1973) Prabhakaran reports their latest findings in a comprehensive review of the activity and function of this enzyme. This work may point the way to a chemotherapeutic agent for treating leprosy.

Tyrosinase is utilised in the oxidation of DOPA (dihydroxyphenylalanine) in the formation of pigment. The enzyme is present in animal and plant cells, and exhibits in *M. leprae* (as in other cells of low phylogeny) a wide substrate specificity. Manometric, spectrophotometric and more recently radio-isotope tracer techniques have demonstrated the rapid oxidation of DOPA in viable organisms isolated from humans and armadillos (the nine-banded armadillo has recently been found to be susceptible to *M. leprae* infections). Certain properties of the bacillary enzyme which distinguish it from the plant and mammalian enzymes have also been intimated.

In contrast to tyrosine of mammalian and plant origin, no substrate inhibition of the bacillary enzyme has been demonstrated and neither is the enzyme inhibited by penicillamine or mimosine. The intermediary quinone produced in the conversion of DOPA to melanin in mammalian and plant cells is dopachrome; in *M. leprae* it is indole-5-6-quinone. In plants and mammals reduc-

ing agents regulate the activity of tyrosine by inhibiting the production of quinone from DOPA, whereas in *M. leprae* the enzyme is not affected (although the further oxidation and polymerization of quinone to melanin is prevented). In plants tyrosine is a soluble enzyme; in mammalian cells the enzyme is particulate and can be released by deoxycholate. In *M. leprae* the enzyme is also particulate and can be released by the anionic detergent sodium dodecyl sulphate but not by deoxycholate. The existence of differences between the mammalian and bacillary enzyme indicates that *M. leprae* enzyme may be a suitable site for the action of chemotherapeutic agents, particularly because it seems to have a key metabolic role in the growth of the bacillus.

Prabhakaran points out that the sites invaded by *M. leprae* in man are all sites where free DOPA is present—skin and peripheral nerves, the eye (ciliary body and iris), testis, adrenal medulla and Schwann cells of dorsal root ganglia of spinal nerves. All these tissues including the melanocytes inhabited by the bacilli in skin and mucous membranes are derived from neural crest cells of the embryo. *In vitro* the production of melanin by melanocytes is suppressed in the presence of viable *M. leprae* organisms, indicating utilization of the DOPA by the bacilli.

A requirement for DOPA would therefore explain why the leprosy bacillus occupies such a unique environment in the human host. The most important implications of these findings, however, are concerned with chemotherapy. Knowledge of biochemical differences between host and causative organism induces a rational approach to the design of chemotherapeutic agents. In the case of leprosy, where an alarming degree of resistance to the sulphone drugs is becoming evident, new chemotherapeutic agents are urgently required.

In this respect inhibitors of *M. leprae* tyrosinase activity may prove to be of great value.

Diethyldithiocarbamate (DDC) was found to be the most potent inhibitor of *M. leprae* tyrosinase both *in vitro* and *in vivo* where it suppresses multiplication of the bacillus in mouse footpads and results in a loss of viability. DDC probably inhibits the tyrosinase by binding with the copper moiety of the enzyme. Acting in this way, any mutation in the amino acid sequence of the enzyme, which is the cause of drug resistance in some instances (for example, in some bacteria resistance is induced in this way to rifampicin, a drug which has recently proved to be of value in the treatment of leprosy), would not result in decreased activity of the drug. The structure of DDC is such that the compound can pass easily through lipid predominant pores of the membrane, so that resistance due to a permeability barrier (as occurs in many instances of bacterial resistance) is unlikely to occur.

DDC and other compounds designed to inhibit the utilization of the apparently essential DOPA may therefore prove valuable in the treatment of leprosy.

NAVIGATION

Homing by Pigeons

from our Animal Behaviour Correspondent

IT is one thing for an animal to be able to take up a consistent orientation and find its way home using a simple compass (Sun, star and magnetic compasses are found throughout the animal kingdom). It is, however, a feat of a quite different order for the animal to be able to home consistently from wherever it is released, that is to say to take up a different compass direction depending on where it is in relation to home. Pigeons are able to do this over long distances

Terminator Anticodons in 18S rRNA

In *Nature New Biology* next Wednesday (October 31) Dalgarno and Shine report that they have sequenced the eight nucleotides at the 3' end of 18S RNA from yeast and *Drosophila* by stepwise degradation, followed by condensation with labelled isonicotinic acid hydrazide, and compared them with the same sequence in rabbit reticulocyte 18S RNA. The sequence, GAUCAUUA_{OH}, was the same for all three eukaryotes. Sequences at the 3' end of RNA isolated from the large ribosomal subunits show no similar homology.

Dalgarno and Shine suggest that conservation of this fragment in such distantly related species implies a particular role in some cellular processes, and advise inspection of the antiparallel

RNA complement of the sequence for an indication as to what this role might be. The complement (UAAUGAUC) contains two sequences, UAA and UGA, which are terminator codons in messenger RNA and one other, UAG, if "wobble" occurs. Since it is known that transfer RNA is not involved in recognising terminator codons, it seems likely that the octanucleotide sequence at the 3' end of 18S DNA molecules has this function.

The authors tentatively propose that the conserved sequence "scans" the mRNA during translation until the 3' oligonucleotide forms base pairs with the terminator codons in the mRNA, at which a factor is activated, so that the ribosome can be released.

and nobody is quite sure how they do it. Keeton (*J. comp. Physiol.*, **83**, 1; 1973) suggests that a valuable clue to this mystery may be supplied by exploiting a curious fact about pigeon homing: when released from a site well away from their home loft, pigeons depart non-randomly, but the mean vanishing bearing almost never coincides with the direction of their home. Instead, the birds set off in a direction which consistently deviates from home and correct themselves later on in the flight. What is more, this initial deviation is often characteristic of a given release site. If these 'release-site biases' are due to some local distortion of geophysical cues that the birds are using, then finding out about the nature of this distortion may help one to understand what navigational system the birds are using.

Keeton investigated a particular release site 89 miles NNE from his lofts in Ithaca, NY, from which his pigeons consistently fly off on a bearing about 60° to the right of the home bearing. By releasing numbers of birds (singly, so they did not influence one another) he found that a similar directional bias was shown by the pigeons whether or not the Sun was visible and whether the birds were experienced homers or on their first flight ever away from the loft. The biasing factor at the release site does not, then, seem to be dependent on weather or on the experience of the birds. Pigeons from other lofts and bank swallows also deviated in the same clockwise direction.

Keeton also found that it is not some feature of the landscape which attracts or repels the pigeons. It is well known that keeping pigeons in an artificial light-dark regime which is ahead or behind that of the outside world will result in their vanishing bearings being shifted to left or right (the 'clock shifting' gives them a false idea of time and upsets their ability to use the Sun as a time-compensated compass). By clock shifting birds 4, 5, or 6 h forwards, Keeton was able to make his birds set off more directly towards home. In other words, pigeons will fly directly homewards if their navigational system tells them that this is the direction to go. Keeton is at present trying to find out what it is about this release site that causes the navigational system to respond inappropriately, for he believes that his birds are not making an error, but are probably taking readings correctly from a distorted 'map'.

COLLOID SCIENCE

Magnetic Filtration

from our Materials Science Correspondent
A GOOD criterion for judging whether a piece of research is suitable for summarising in News and Views is the difficulty

found in choosing a heading for the possible story. This one is headed 'colloid science', but it might equally well have been 'applied physics', 'magnetism', 'fluid dynamics', 'filtration' or 'pollution abatement'. Interdisciplinarity is all the rage, and is common enough in teams: what is much rarer, and apt to get results, is the one-man interdisciplinary team.

J. H. P. Watson is one of these monanthropic teams. He has just outlined (*J. appl. Phys.*, **44**, 4209; 1973) the principles of a new method of filtration. He examines the behaviour of a suspension of small paramagnetic spheres in a liquid flowing at right angles to a ferromagnetic wire when a steady magnetic field is applied parallel to the direction of fluid flow. Briefly, the particles are attracted to the wire and will adhere to it if they pass within a critical distance which is itself a function of flow rate, wire diameter, particle diameter, ferromagnetic permeability and paramagnetic susceptibility. The most important parameter or figure of merit seems to be the ratio of the fluid velocity to a 'magnetic velocity', which unfortunately is nowhere defined or explained.

Watson goes on to consider the efficiency of a filter of crumpled wire in a field, in terms of the packing fraction and the above-mentioned figure of merit, both for streamline flow and for the less tractable case of turbulent flow. Such a filter, given a sufficiently strong field and fine particles, could be highly efficient, a kind of liquid-flow analogue to the electrostatic precipitators used for smoke. Watson considers the special case of sewage. Here it would be necessary to use paramagnetic powder, such as haematite, as "nuclei for activated-sludge flows", and then to extract the powder from the filtrate and recycle it. Watson claims that such a filter would be 100 times more efficient than a sand filter of the same area—perhaps a rather modest factor of improvement in view of the probable capital and process costs, but interesting nonetheless. He suggests that a superconducting magnet would be needed to attain a sufficiently high field (no field-strength is quoted anywhere), and that a 12-foot diameter magnet/filter could process 2.5×10^7 gallons a day and serve a city of 0.5 million. The economics of this proposal would be intriguing in the extreme—it is hard to see whether it would be an environmentalist's Concorde or an ultra cost-effective vehicle for recycling haematite, liquid helium and water.

The particular application proposed may provoke a measure of scepticism and the theoretical treatment, as published, may raise a number of unanswered questions, but the filtration technique is one with most interesting potential, especially if the theory can be experimentally confirmed.

PLATE TECTONICS

Subduction Reverses

from our Geomagnetism Correspondent

ONLY a few years ago, heat flow studies were often regarded as the poor relation of the geophysical family; the results were interesting but it was difficult to see what conclusions could be drawn from them. The most striking fact to have emerged in the early days of heat flow work was that the average continental and oceanic heat flows are approximately equal, and this was felt intuitively to be important even though its implications were far from clear. But even this unexpected discovery had little impact on the mainstream of geophysical thought, precisely because of the problem of interpretation. Today, however, the situation is quite different. For one thing, Sclater and Francheteau (*Geophys. J.*, **20**, 509; 1970) have shown convincingly that the continental-oceanic equality is largely irrelevant because it fails to take account of variations in heat flow with age. But on the more positive side, heat flow data are now playing a much more significant part in the detailed interpretation and clarification of plate tectonic processes.

A good example of this is provided by new and old heat flow results from the western equatorial Pacific (Halunen and Von Herzen, *J. geophys. Res.*, **78**, 5195; 1973). The equatorial Pacific west of Hawaii is a large and complex area still very poorly understood in plate tectonic terms and for which geophysical data are still relatively sparse, although its extensive arc-trench system is generally believed to indicate a set of convergent plate boundaries. But in this general scheme of things, the Solomon Islands region is apparently anomalous. In much of the western equatorial Pacific the island arcs are associated with trenches and subduction zones produced by lithospheric spreading from the Pacific side; and on this basis the Solomon Islands, which stretch out in a roughly northwest-southwest direction, would be expected to have a trench on the northeast side. But this is not so; the trenches in this region lie on the continental side of the island arc system.

To help explain this apparent discrepancy, Halunen and Von Herzen have turned to good advantage the Earth's high thermal inertia, a phenomenon which is often regarded as more of a hindrance but which in this case enables recent changes in plate tectonic behaviour to be identified. Combined newly and previously obtained data show that immediately northeast of the Solomon Islands the heat flow is low at about 35 mW m⁻² but increases to 'normal' (about 64 mW m⁻²) out in the Pacific to the north. To the southwest of the islands, on the other hand, heat flow is high at about 83 mW m⁻², with two

particularly high values reaching 126 mW m^{-2} and 143 mW m^{-2} . Further to the south of the Solomon Islands, in the Coral Sea basin, heat flow reverts to normal (here about 62 mW m^{-2}).

Generally speaking, heat flow in the vicinity of a trench-island arc system is about normal beneath the ocean basin adjacent to the trench, low in the trench region where lithosphere is descending, and above average on the continental side. The reason for the high heat flow behind arc-trench systems is still a matter of debate; but for the present purpose it is sufficient to know that it is a feature of island arcs clearly related in some way to the plate tectonic processes involved. In the Japan trench-arc system, for example, heat flow in the Japan Sea, the continental side of the system, averages 84 mW m^{-2} and that in the trench itself averages 29 mW m^{-2} . These figures bear a notable resemblance to those from the Solomon Islands system. The only problem is that in the Solomon Islands case the heat flow values are reversed, being high in the trench side (although low in the trench itself) and low on the continental side. In other words, the heat flow data are apparently more consistent with the expected lithospheric downwelling from the Pacific side than with the actual descending of the lithosphere from the Australian side.

This is where the point about thermal inertia comes in. Halunen and Von Herzen argue that because of the Earth's high thermal inertia, the heat flow observed now (apart from the present trench) reflects not the current tectonic situation but that existing at some time in the past (probably the upper Miocene). Notwithstanding the apparent anomalous behaviour beneath the Solomon Islands region in the context of the western Pacific as a whole, the tectonic situation is, in fact, consistent with plate tectonics which shows that in the southwest Pacific the Australian plate is moving northwards and underthrusting the Pacific Ocean plate. The case now put forward by Halunen and Von Herzen, however, is that this is a comparatively recent development, and that at one time subduction took place from precisely the opposite (Pacific) direction in accord with the observed heat flow. They then go on to suggest that what reversed the flow was a particularly thick (about 40 km) piece of spreading oceanic lithosphere which collided with the original island arc and, because of its buoyancy, effectively prevented further subduction from the Pacific direction. Because a continuation of lithospheric consumption was essential, subduction then began from the Australian plate. The crustal block which stopped the original subduction now largely forms the Ontong Java plateau which lies to the north of the Solomon Islands.

This model not only explains the observed heat flow but is also consistent with other geological and geophysical features of the region. For example, Coleman (*Pacif. Sci.*, **24**, 289; 1970) used structure and rock types to divide the Solomon Islands system into three linear geological provinces along the trend of the arc—the Pacific, central and volcanic (southwest) provinces. Of these, the central and volcanic provinces are typical of other western Pacific island arc systems. But the Pacific province is not, Coleman's conclusion being that it did not exist in its present position prior to the upper Miocene. To this, Halunen and Von Herzen add that the Pacific province is probably part of the Ontong Java plateau and thus did not assume its present position until about the time the subduction was reversed.

On the geophysical side, the shallow seismicity is, as would be expected, consistent with the present pattern of lithospheric motion. Shallow-earthquake epicentres lie along the south coasts of

the Solomon Islands; and the distribution of intermediate foci suggests a subduction zone dipping at about 45° to the northeast. But there is a gap in foci at depths of 225–380 km and the resumed deeper activity indicates an almost vertical focal plane. This suggests a detached slab of lithosphere at depth, although Halunen and Von Herzen propose that the slab may be not part of the present descending lithosphere but a remnant of the original Pacific subduction. The pattern of attenuation of short period S waves is, as is usual in island arc systems, higher across the continental side and lower across the oceanic side; but, of course, since the subduction behaviour beneath the Solomon Islands is opposite from that of the western Pacific as a whole, the attenuation pattern is really more consistent with the original direction of subduction.

In conclusion, as Furumoto *et al.* (*Pacif. Sci.*, **24**, 315; 1970) demonstrated, the crust is indeed unusually thick (35–42 km) beneath the Ontong Java plateau.

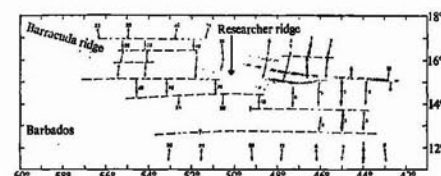
Magnetics East of the Lesser Antilles Arc

In spite of the widespread attention given to oceanic magnetic anomalies in recent years, there are still many areas where anomalies are poorly defined, if at all. In some cases it is simply a question of waiting for somebody with sufficient resources to take an interest; but some of the regions remaining unstudied also present particularly difficult problems.

In the section of the Atlantic Ocean east of the Lesser Antilles island arc, for example, the magnetic pattern is complicated by the large number of fracture offsets, and the identification and correlation of lineations are made more difficult by a combination of slow spreading rate, relatively low anomaly amplitude and topographical influences. Nevertheless, in *Nature Physical Science* next Monday (October 29), Peter *et al.* are able to report that they have identified the whole Cainozoic magnetic anomaly pattern in the area, and conclude from it that the evolution of the corresponding section of the mid-Atlantic ridge was little different from that elsewhere in the north Atlantic.

This conclusion is based largely in a correlation of the magnetic anomalies and topography of the mid-Atlantic ridge east of the Lesser Antilles island arc with those of a standard ridge section between the Atlantis and Kane fracture zones in the north Atlantic. This correlation clearly shows that properties considered typical of the north Atlantic extend southward to the Vema fracture zone. Even so, some results were unexpected. For one thing, the offsets (dashed lines on the map) trend east-west. Moreover, the continuity of

lineations across the trend of the Barracuda ridge ($N 108^\circ E$) suggests that this ridge is not part of a major transform fault but only a local feature. Similarly, although the southernmost of the three major fault troughs (hatched regions) coincides roughly with an offset and correlates with the east-west offset across the Researcher ridge, the two northerly troughs are not associated with magnetic offsets. The northerly troughs and parts of the ridges are thus probably the results of normal or reverse (rather than transform) faulting. North of the area of investigation, offsets do strike northwest-southeast, however, the change in direction probably occurring at about $19^\circ N$ where the trend of the ridge crest also changes direction.



Finally, Peter *et al.* contend that their data support the view that since the upper Cretaceous North America and South America have moved away from Africa essentially as a single plate. They also propose a modification to the pre-drift reconstruction of the north Atlantic which would require a less severe rotation of the central American Palaeozoic blocks and allow for an east-west spreading of the mid-Atlantic ridge between Africa and the central American blocks from the upper Triassic to the lower Cretaceous.

SOLID STATE DEVICES

Progress Still Rapid

from our Solid State Physics Correspondent
INTRODUCING his technical paper at the Third European Solid State Device Research Conference in Munich on September 21, G. H. Schwuttke (IBM) informally sounded the keynote of the conference when he reflected on the rate of development and cost improvement in solid state device technology. He said that, if the motor car had developed with the same rapidity, our vehicles would now cost £100 and travel at 10,000 miles an hour. His point was that demands on the performance of materials have become severe as device dimensions and rates of operation have changed by the orders of magnitude implied in his example. The strongest demands have been in purity, perfection and fineness of structure in the intricate patterns of interwoven solid phases which go to make up a solid state device.

Compared with earlier conferences perhaps the greatest difference was the upsurge of contributions on means of forming arrays of metal-oxide-silicon structures, using techniques such as ion implantation and growth of epitaxial silicon films. For example, F. F. Fang and H. Rupprecht (IBM) investigated the ultimate switching speed for a chain of MOS gates, using all the 'tricks' of implantation techniques. They achieved delay times as low as 0.1 nanosecond and standby power drains as low as 2 nanowatts from all-n-channel arrays. R. de Werd and I. A. den Boer (Phillips) described improvements imparted by implantation to the already sophisticated LOCOS methods for making complementary-symmetry MOS circuits. Three papers were also given by investigators from Siemens on complex circuits made from silicon on sapphire; this medium will eventually allow even faster switching times by reducing capacitances but control of the film properties is still primitive.

Working alongside the people developing metal-insulator-semiconductor devices, some physicists have been trying to understand charge transport and buildup in the MIS oxide layer: G. Sixt (Institut für Festkörperphysik) and A. G. Holmes-Siedle (University of Reading) described how independently they have used high-energy light in different ways to characterise charge profiles in bombarded insulators. The floating gate MOS memory has also been improved by ingenious use of charge injection into insulators. Eschewing light, J. Verwey (Phillips) has used junction avalanche effect to inject holes across the silicon dioxide layer; H. Card has used Poole-Frenkel conduction through silicon nitride for the same purpose. M. A. Green and J. Shewchun (McMaster University, Ontario) have also used

oxide transport and they demonstrated a useful range of operating modes for their MIS tunnel diode, first discussed last year; in many respects, this device can mimic a p-n junction but the problems of forming a metallurgical junction are avoided.

In the junction device field, it is clear that much still has to be found out about the way in which the p-n junction operates, especially when used at the limit of its performance. At high currents, minority carrier injection levels are such that new forms of recombination occur. An example is impact recombination, which is an Auger effect. In this regime, the ratio of diffusivity to mobility deviates from the conventional Einstein relation; N. G. Nilsson (Royal Institute of Technology, Stockholm) has developed a suitably generalised Einstein relation, and K. Roy and K. H. Glöckner (AEG Telefunken) and J. Cornu and M. Lietz (Brown Boveri) have investigated this phenomenon in high voltage P-I-N diodes. Work on gallium arsenide devices continues to be intensive; for example, in the information handling area, H. Kleinknecht (RCA) reported 70% modulation of He-Ne laser light in GaAs epitaxial light guides, using the Pockels effect. There was also a whole session on light-emitting diodes and

another on Gunn devices. It is clear that, for the sake of smoothing technological progress, more has to be found out about the physics of the III-V compounds, including their band-to-band transitions, mechanisms of degradation under stress and the defect structures of real crystals. As mentioned, device performance is increasing rapidly. Schottky-barrier field-effect transistors in GaAs can operate in the millimetre wave region and P. A. Kirkby and G. H. B. Thompson (STL) have achieved an output of 40 W mm⁻¹ from heterostructure lasers.

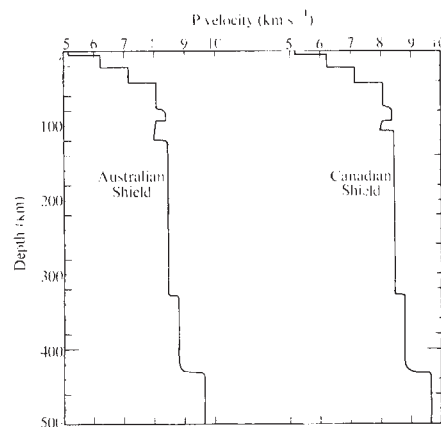
Opto-electronic devices probably provided the peak of interest for the conference. J. Carnes (RCA) gave a riveting survey talk on charge-coupled devices and announced a 160 by 128 imaging array which could act as a pickup device for television display while accommodating the usual interlace delays; the publicity release talks about a camera the size of a cigarette packet. For the first time, liquid-crystal displays were admitted into an otherwise purely 'solid state' conference. This is reasonable on several grounds, not the least being the general opinion expressed by speakers that reflective liquid crystal arrays will probably carve out a large share of the display device market.

P Wave Velocities and Plate Motions

ALTHOUGH the precise mechanism by which horizontal lithospheric motion takes place is still uncertain, it is generally agreed that the lithosphere is able to move by virtue of the low velocity channel, or asthenosphere, which underlies it. The asthenosphere is the weak zone which reduces the coupling between the lithosphere above and the mesosphere below. But if this is the case, argues Massé in *Nature Physical Science* next Monday (October 29), it is probable that lithospheric motion is influenced by the thickness of the asthenosphere, which is known to vary around the Earth. In particular, it is likely that, for a plate containing con-

tinents, the greatest resistance to motion will occur beneath the shield regions where the low velocity channel is thinnest. This, in turn, implies that the rate of movement of a plate which includes a continent may also be influenced largely by the thickness and nature of the low velocity channel beneath the shield.

But is there any evidence to support such a view? Much more needs to be known about the lithospheric driving force before final conclusions may be drawn; but in the meantime Massé notes that a comparison between the Canadian and Australian shields gives general support to his hypothesis. The P wave velocities in the low velocity zones beneath these areas are equal (see diagram) implying similar physical constitutions for the two zones. But the thicknesses of the zones are different, and so are the drift rates of their respective continents. In numerical terms, the thickness of the low velocity channel below the Canadian shield is only 0.5 times that of the channel beneath the Australian shield and, according to Minster *et al.* (*Eos*, 54, 238; 1973), the absolute drift rate of North America is only 0.4 times that of Australia. Thus although the analysis is limited to a simple comparison, the relevant parameters are certainly consistent with Massé's hypothesis.



Measuring the Class Structure

A. STEWART, K. PRANDY & R. M. BLACKBURN

Department of Applied Economics, University of Cambridge, Cambridge CB3 9DE

Previous discussions of social class have assumed a hierarchical structure to social relationships. Using recently developed computer techniques, it has been possible, for the first time, to test this assumption. An analysis of a survey of friendship choices indicated a probable one-dimensional structure.

THIS article is a short report of research using recently developed methods in a study of what has usually been called "social class" or "social status". In addition to the substantive results, one of our aims is to demonstrate the utility of advanced methods for treating data in the social sciences. In particular we are concerned with demonstrating a structure from social relationships and in doing so we wish to illustrate how, in general, structures at an aggregate level may be developed out of large numbers of individual actions, even where the individuals concerned are unaware or only partially aware of such structures.

In both the sociological literature and common-sense views of society it has generally been assumed that social relationships are structured in a hierarchical way: in other words, that the quality of social relationships between individuals depends on their positions in a hierarchical structure. A particular implication of the existence of such a structure is that interactions between individuals at the same level within it are on a basis of equality. This has generally been linked to the concept of "status", where it is assumed that those of similar status, frequently represented as social honour or prestige, choose to interact with each other socially, and that life styles develop which are common to status equals and exclusive of status inferiors. Other writers have rejected differential prestige as a major basis of stratification, but have nevertheless accepted a similar hierarchical structure. This being so, in spite of important disagreements about the determinants and nature of stratification, the actual measurements of it have, in practice, been similar. Occupations have been used as a basis for differentiating populations with the assumptions that an ordering of occupations will reflect the hierarchical structure. The differentiation has frequently been of a very simple form, as for example the distinction between manual and non-manual occupations, but also it has often entailed more elaborate divisions, such as the definition of five classes by the Office of Population Censuses and Surveys² which is perhaps the most approved and used measure of stratification in Britain.

In keeping with this consensus we also used occupations to differentiate our population, but we made no initial assumption of ordering. Our aim was, first, to test what has previously been assumed by writers of varying theoretical orientations: that there is a specific hierarchical structure to be found in social relationships. It is important to recognise, however, that our methods test directly only the existence and dimensionality of a structure. The question of whether a unidimensional structure is hierarchical in nature must be determined by external criteria, but insofar as we have predicted the structure on the basis of theories involving a hierarchy, finding a unidimensional structure may be regarded as persuasive evidence for it. A second aim was to develop a measure of the hierarchy, if it exists. As a consequence of our finer occupational distinctions and of the precision of our methods we hoped to provide a more extensive and sensitive measure than had previously been available.

Grading Occupations

The manual/non-manual dichotomy and the five-class division mentioned above are products of the intuition of their creators and the only test of their validity is their plausibility and utility. Where more subtle distinctions have been required, researchers have been more self-conscious about methods and have usually created these distinctions from the perceptions of samples of the population, arguing their validity from the representativeness of the samples. Most frequently respondents have been asked to rank or rate a list of occupations and their answers have been aggregated in some way to produce a scale. There are many weaknesses in these approaches, not least the fact that a rank order of some kind (that is, a single dimension of "occupational status") will, by most methods, necessarily be obtained irrespective of the variations in the perceptions of respondents. In this study we decided that, rather than ground our research in an artificial procedure of having respondents rate occupations abstracted from their context, we would choose as our basic data actual social relationships (preferred leisure-time interactions), and rather than adopt methods that would prejudge the issues of dimensionality and coherence we would use multidimensional scaling techniques to extract the inherent regularities of patterns of interaction without any previous assumption of structuring.

Computer programs for multidimensional scaling techniques have been developed in the past 10 yr³⁻⁵. These are techniques for extracting metric results from non-metric data, which would include most data in the social sciences, and they also provide ways of estimating the dimensionality of data. The program we

Table 1 Occupations and Occupational Scores for Respondents' and Friends' Solutions

Respondents and friends	* Scores	†
Warehousemen	-188	-164
Guards and related workers	-183	-151
Foremen Engineers (so described)	-176	-204
Foremen Chemical Workers	-175	
Foremen Rubber Workers	-159	-193
Foremen Shoe Workers and Leather Workers	-145	
Foremen Non-Skilled Engineering Workers	-140	-103
Foremen Glass Workers	-112	
Foremen Skilled Engineering Workers	-112	-118
Foremen Warehousemen	-97	
Technical and Engineering Assistants	-96	
Foremen Inspectors in Engineering	-93	
Supervisory Laboratory Assistants	-93	
Laboratory Assistants	-78	-16
Clerks	-69	-108
Draughtsmen	-62	-29
Office Managers	-52	-108
Technicians	-41	3
Supervisory Clerks	-37	-84
Supervisory Technicians	-24	
Supervisory Draughtsmen	-6	
Service Managers in Industry	10	35
Accountants	15	14
Civil Service Executive Officers	17	-21
Social Workers	22	-6
Managers in Engineering	25	53
Personnel Managers	32	94
Mechanical Engineers	35	92
Architects and Surveyors	39	67
Managers in Industries other than Engineering	62	46
Electronic Engineers	65	123
Sales Managers	85	83
Chemists	86	27
Professional Workers NEC	95	83
Physical and Biological Scientists	101	97
Senior Officials in National Government	103	43
Technologists	104	102
Electrical Engineers	129	129
Senior Officials in Local Government	132	95
Civil Engineers	139	72

* First dimensional values derived from similarities of respondents' occupations.

† First dimensional values derived from similarities of friends' occupations.

Product moment correlation between columns 1 and 2 = 0.93.

have used positions N points in an m -dimensional space, in such a way as to achieve the best possible approximation to a monotonic relationship between the $(N/2)$ interpoint distances in that space and a ranking of the observed dissimilarities among all pairs of N objects. Kendall⁶ demonstrated that computer programs for these techniques can produce very accurate maps from apparently inadequate sets of facts.

Analysing Responses

The respondents in our study were 1,922 white-collar workers in occupations ranging from foreman and clerk to senior manager and professional, employed in thirty-two varied establishments within a sixty mile radius of Cambridge. In the course of an interview primarily concerned with aspects of white-collar work they were asked to give us the occupations of four people with whom they were friendly outside work. They were encouraged to give occupations other than their own. Both our respondents and their friends were classified according to the *Classification of Occupations 1966*, published by the Office of Population Censuses and Surveys. For present purposes the forty occupations with ten or more respondents were extracted

from the data. The dissimilarities between each pair of occupations were calculated using an index of dissimilarity used in a number of other studies using multidimensional scaling techniques. It involves, in our case, taking the sum of positive percentage differences between the distributions of friendship choices for pairs of occupations. Where there is a complete overlap of friendship choices the index of dissimilarity has a value of 0 and where there are no friendship choices in common it has a value of 100. The matrix of dissimilarities between all pairs of our forty occupations was fed into the MINISSA program developed by Lingoes and Roskam (Roskam E. E. Ch. I., and Lingoes, J., *Non-Metric Multidimensional Scaling GCLR MINISSA—I Standard Version* (computer program available from Roskam and Lingoes) (1969)).

The results are given in Table 1. We shall go on to argue that there is a single large dimension in our data, and the first column figures anticipate that argument. They are the best approximations to values on that dimension for the forty respondents' occupations. Included in Table 1 are the results from a second way of approaching the question of structure. Instead of occupations of respondents arranged by similarity of friendship choices, it is possible to arrange occupations of friends according to the respondents who have chosen them. Our respondents chose friends in many different occupations (partly as a result of our encouragement to choose occupations other than their own) and we were able to isolate eighty-five occupations containing twenty or more friends. The results are similar in important respects to those for respondents and the technical comments which follow, though they are made only in relation to the latter, might equally apply to both. In substantive terms the friends' result is of considerable interest (it allows us, for example, to examine the positions of manual as well as non-manual occupations and indicates that there is almost no overlap between them), but for present purposes we shall be concerned only with those thirty-one occupational groups which are shared with the respondents' solution. The values on the friends' solution are given in the second column.

Determining Dimensionality

The most frequently used statistic for evaluating results is Stress, which is a measure of the deviation of the derived solution from a perfect fit with the original data, and is similar to a residual sum of squares. Rough guides to the interpretation of Stress values were suggested by Kruskal⁷ and endorsed by Roskam⁸. Several more recent works^{9,10} which take account of the number of points to be placed in the space have, however, shown the limitations of these rules and have suggested more refined criteria. There is no doubt that our results are highly significant according to these latter. Every one of our solutions in from one to six dimensions is highly significant, however, and a problem that arises is the status to be given to the solution for dimensions above one or, in other words, how to assess the "true" dimensionality of the structure. If in our case, for example, the interpoint distances are an inaccurate reflection of a single dimension (accounted for by one dimension plus a large number of errors each affecting only small parts of the data) then an improvement in Stress would still be found with increases in dimensionality despite the "random" nature of the errors. The extent of the expected improvement, however, is unknown. Kruskal¹¹ has suggested that an indication of dimensionality may be gained by examining the Stress values for solutions of increasing dimensionality for an elbow effect: a sudden decline in the improvement of Stress values. Dimensions at and after the elbow would be regarded as non-significant. Unfortunately elbow effects do not always occur and Klahr¹² has shown that they can occur merely as a consequence of noise, though this is more likely when there is a small number of points.

Since an examination of Stress values is therefore unlikely

alone to yield a satisfactory approach to the dimensionality of data we have chosen to look in addition at differences in the proportion of variance due to each dimension in our various solutions. (In general one must approach with caution an interpretation of data which takes the proportion of variance due to dimension of a solution as an indication of the size of a substantive dimension. Monotonic distortion can add dimensions to solutions and also increase the size of dimensions. This problem is particularly acute in a large dimensionality with a small number of points to be fitted. In this case it is of little importance.) The results are presented in Fig. 1.

The strong elbow effect in this diagram indicates that in all solutions the first dimension is very much larger than all succeeding dimensions. On the other hand, the second and subsequent dimensions do not show the same relation to succeeding dimensions. In each solution in from three to six dimensions the second dimension has little more of the variance than the third dimension and so on.

In addition, the first dimension does not change much from solution to solution (the product moment correlation between the one-dimensional solution and the first dimension of the six dimensional solution is 0.93), though it becomes increasingly consistent with increasing dimensionality (the one-dimensional solution correlates 0.97 with the first of two dimensions, the first of two dimensions 0.99 with the first of three). The second and subsequent dimensions do not display this regularity.

The evidence then is that there is one large dimension in our data, plus a good deal of noise. Some of the noise we know is associated with the measure of distance between occupations we have used and with better measures it should be reduced. Meanwhile we have chosen to regard the first dimension of the six-dimensional solution, corrected for distorting factors in the method, as the best approximation to our large dimension. The distorting factors are too complex to be discussed here, but their major consequences are overestimations of the distances between adjacent points at the extremes of the first dimension.

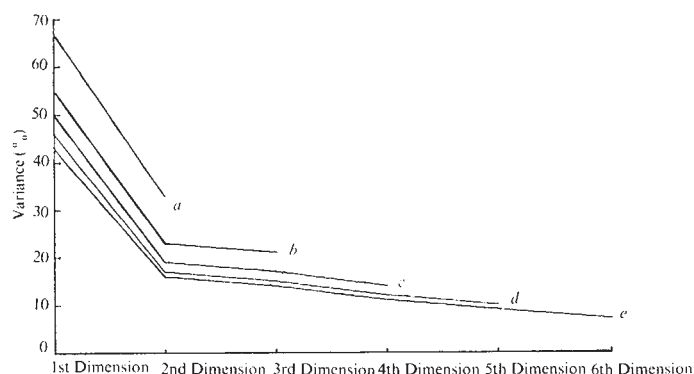


Fig. 1 a, Two-dimensional solution; b, three-dimensional solution; c, four-dimensional solution; d, five-dimensional solution; e, six-dimensional solution.

The scores in Table 1 are adjusted to take account of this. The order of occupations remains unchanged.

The product moment correlation between the respondents' solution and the friends' solution is 0.93. We regard this as satisfactory, especially in view of the disadvantages of the occupational groupings we have used. Some of the occupational categories we have taken over from the "Classification of Occupations" are disturbingly wide in the range of specific occupations they include (for example artists, sculptors, engravers and window dressers come in the same category). This being so, there are occasions when an occupational grouping of our respondents is not identical with the equivalent occupational

grouping of friends. The fact that the solutions are so well related is, in the circumstances, strong evidence of a regular structuring.

Comparison with Other Data

Although we have argued that the methods of this study are the most appropriate to work in this area, it is also of interest to see how our results compare with those obtained by other methods. First, they are in large part consistent with previously constructed measures (they distinguish routine non-manual workers from managers and, for the most part, managers from professionals), but we are more concerned with the relationship to scales resulting from more rigorous techniques. Unfortunately there are no extensive measures produced in Britain which are suitable for the purpose. In fact the only one we could find with sufficient overlap with our data was Duncan's Socio-Economic Index¹³ for occupations in the USA. This measure is a weighted combination of the percentage of members of occupations who were high school graduates and the percentage of members of occupations who were earning over \$3,500 in 1949. The data are available from the United States Census, but equivalent data are not available for Britain. The weightings used were obtained from the regression of the education and income measures on a prestige scale created from evaluations of occupations by a national sample of the population of the USA. The latter was developed by the National Opinion Research Center¹⁴ (NORC), and covered ninety occupations. The occupational titles are different from those used in this country and the overlap between our scale and the NORC scale was not sufficient to allow us to correlate them. The Duncan Socio-Economic Index can be extended to cover many more occupations and we have been able to find twenty-eight occupations in common. In spite of the lack of identity in occupational titles between the scales, and in spite of the fact that they were developed in different countries, the correlation is 0.90. Coincidentally, that is exactly the correlation between Duncan's Socio-Economic Index and the NORC scale.

The relationship to other measures is interesting, not least because they were created to measure an assumed hierarchy. Adequate tests of the hierarchical nature of the structure, however, will require further analysis of the relationship between position in the structure and its consequences for social experience. We are at present engaged in a study which will increase the range of occupations covered, and will also examine some of the determinants of stratification and the nature of experience at various points in the social structure. In the meantime we offer these results as evidence of such a structure and as a means of measuring part of that structure.

The data for this research were a small part of the total data of a project financed by the Social Science Research Council.

¹ General Register Office, *Classification of Occupations* (HMSO, 1966).

² Kruskal, J. B., *Psychometrika*, **29**, 1 (1964).

³ Kruskal, J. B., *Psychometrika*, **29**, 115 (1964).

⁴ Guttman, L., *Psychometrika*, **33**, 469 (1968).

⁵ Roskam, E. E. Ch. I., *Metric Analysis of Original Data in Psychology* (Vam-Voorschoten, 1968).

⁶ Kendall, D. G., *Nature*, **231**, 158 (1971).

⁷ Kruskal, J. B., *Psychometrika*, **29**, 1 (1964).

⁸ Roskam, E. E. Ch. I., *Metric Analysis of Ordinary Data in Psychology* (Vam-Voorschoten, 1969).

⁹ Stenson, H. H., and Knoll, R. L., *Psychol. Bull.*, **71**, 122 (1969).

¹⁰ Klahr, D., *Psychometrika*, **34**, 319 (1969).

¹¹ Kruskal, J. B., *Psychometrika*, **34**, 319 (1969).

¹² Klahr, D., *Psychometrika*, **29**, 15 (1964).

¹³ Duncan, O. D., in *Occupations and Social Status* (edit. by Reiss, A. J.), Chap. 6 (Collier-Macmillan, 1961).

¹⁴ *Occupations and Social Status* (edit. by Reiss, A. J.), Chaps. 1-5 (Collier-Macmillan, 1961).

Educability and Group Differences

J. M. THODAY

Department of Genetics, Milton Road, Cambridge CB4 1XH

Professor Thoday reviews a recent book by Arthur R. Jensen.

THE recently published book by Jensen¹ is a very difficult one to review, not so much because the subject is controversial, but because so much of opinion in the area is strongly held, so that critical evaluation of the relation of data to hypothesis is limited. Thus, to review the book properly one would need not only to consider other books that have resulted from the publication of Jensen's article² in *Harvard Educational Review* but also to consider each important statement in each book in relation to the original literature on which the statement is based. Even this would be inadequate, for it is my experience that the original sources seldom give enough information about sample structure or detail about results and statistical techniques to permit satisfying judgment and that correspondence with the authors of the paper is needed.

No reviewer can do all this especially in such an interdisciplinary field. If one does any of it, however, one is brought face to face with a most unsatisfactory situation: a scientific area in which, because of the impact of strongly felt ideopolitical points of view, there is wholly inadequate objective criticism of the relation between data and conclusions either by authors or their readers.

Jensen, in his original *Harvard Educational Review* article², raised old questions because he was, I am sure rightly, disturbed that much literature, and a good deal of policy, is based on the assumption that variance in performance of IQ tests, and thus in correlated aspects of educability, arises largely or wholly from environmental reasons, despite the strong evidence for a large within-group genetic component. He also raised the question of between-group differences, in particular the average difference between American white and negro social groups, maintaining that it was plausible to postulate a genetic component of this difference also. It should be stressed that Jensen only put this forward as a hypothesis, not as proved, but it was for putting forward this hypothesis that he has been most attacked. It should, however, be remembered that to postulate genetic variance in 'educability' even in a non-racial context is unacceptable to many.

(The groups mentioned in the previous paragraph are primarily social groups, though they correlate partially with biological ancestry. Leach, reviewing this book in the *Listener*, made this a major criticism of Jensen, holding that he must not make a biological comparison between two groups unless the groups are defined by purely biological criteria. This is illogical. The whole question is whether two socially defined groups differ biologically in a certain variable or whether the difference is all a consequence of social factors.)

Jensen's book is about group differences, but is, as was his original article, based on the evidence concerning within-group differences. Here there are two points which may be regarded as well established.

(1) Relative performance in IQ tests correlates with relative performance in aspects of education that are highly relevant to success in and the needs of modern societies. This statement remains true even if we recognise that relative IQ is not to be equated with relative 'intelligence', and that the

related aspects of educability are not to be equated with the whole of educability.

(2) Within-group IQ variance has a large genetic component. There is room for argument whether Jensen's estimate of 80% may not be on the high side, but there can be no doubt that heritability of IQ ranks high compared with heritabilities reported in the generality of studies of continuous variables, whether behavioural or physical, in man or other organisms.

The high within-group heritability of this variable does not necessarily imply anything about between-group differences, but such differences need explanation. There are, however, fundamental methodological problems, of which Jensen is well aware, which arise from the necessary fact that different social groups differ in their environmental circumstances and are also partial genetic isolates. Any environmental differences are therefore necessarily correlated with possible gene frequency differences and vice versa. Particularly critical experimental design and cautious interpretation are therefore required in this field.

The two main kinds of between-group difference of mean IQ are those concerning socioeconomic groups (or social 'classes') and those concerning 'racial' groups, particularly the United States white-'negro' comparison (I place negro in inverted commas since the United States negro population has much white ancestry).

Considering within-race socioeconomic groups (the correlation between IQ and 'class' is about 0.5), since (among United States and British whites) within-group heritability is high, social mobility is high and social mobility is correlated with IQ, it would be very surprising if gene frequency differences were not the explanation of part of the differences of mean IQ. This, however, is a long way from proof, still less estimation of the relative magnitude of the genetically and environmentally caused components of the group differences.

In his book Jensen gives little attention to 'social class' differences. His one argument about the evidence (pages 155-6) is, however, weak, for though he shows certain correlations are incompatible with a zero correlation between IQ genotype and social class, he makes no allowance for error in the estimates, and the correlations would be compatible if, for example, IQ heritability were 70% instead of the 80% he takes.

Three Questions

Most of his book concerns the 'racial' comparison. The essential fact here is that United States 'negroes' score on the average one standard deviation below the United States white mean in IQ tests.

Three questions can be asked about the causation of this difference. Is the difference (in whole or part) a result of gene frequency differences? Is the difference a result of environmental (including cultural) differences in factors that also vary within the groups? Is the difference a result of some environmental-cultural factor exclusive to the United States 'negro' population?

I shall consider the second question first. It is, of course, clear that there are within-group environmental variables that affect IQ. Otherwise within-group heritability would be 100%, and the correlation between identical twins would be 1. That some of this non-heritable variance is associated with socioeconomic group is strongly indicated by Skodak and Skeel's study which showed that adopted children from

mothers of low IQ had higher mean IQs than expected on a purely genetic model. Jensen deals with this in a note to chapter 11 (page 241).

The negro population is, of course, low in mean socioeconomic status (SES), so that it is reasonable to postulate this as an environmental factor accounting for some of the group difference. Attempts to equate for SES have, however, failed to account for much if any of the difference between white and negro social groups. Likewise, though extreme malnutrition can lower IQ, attempts to investigate effects of nutrition in the United States show negative results (malnutrition at levels found, say, in Africa is, of course, irrelevant to an assessment of the United States negro-white difference). Likewise, most of the other often postulated environmental factors, motivation, reaction to race of tester, language deprivation, inequalities of schooling, culture loading of tests, seem unable to account for the difference. Indeed, when tests which are regarded as differing on culture loading are used, quite the opposite result than that predicted on the culture loading hypothesis is obtained. The white-negro difference is least on the most culture loaded tests. Furthermore, other 'racial' minority groups have higher average IQ than United States negroes, some higher than United States whites. Having considered the evidence, Jensen regards the evidence relating to my second question as quite inadequate to account for the negro-white difference in the United States.

This brings me to the third question, which is, as Jensen puts it, to postulate an unknown factor X. This has to have certain negative features if it is to fulfil its postulated function, for it must be a factor that does not affect minority groups other than negroes, for these do not show the low IQ mean that the 'negro' social group does, nor can the factor be one that affects 'deprived' whites. It has to be exclusive to 'negroes'.

It needs, however, to be pointed out that there are cultural factors exclusive to this negro social group, such as awareness of negro or slave ancestry and awareness of real or supposed social attitudes to 'negroes', and it is not reasonable to discount these simply because they present hypotheses difficult to test, and because some other hypothesis fits the data. Jensen seems to do this.

For example, in arguing for the plausibility of the genetic hypothesis Jensen puts considerable stress on the argument from regression. This argument goes as follows. The 'white' social group has a mean IQ of about 100, variance about 15² and heritability 80%: the 'negro' social group has a mean one standard deviation lower, about 85. Then if we take a sample of whites of IQ about 120, an unbiased sample of their sibs must regress to the mean IQ of 100, that is, genetic theory requires that the sib mean should be about $100 + 20 \times 0.5 \times 0.8$ or 108. (20 is the deviation from the mean, 0.5 the genetic correlation between sibs, 0.8 the heritability.) This it does, which is not surprising since it is part of the evidence that heritability is 80%. By contrast the sibs of a sample of negroes of IQ 120 should regress to a mean IQ of 85, that is, assuming that heritability within the negro social group is also 80% then it should be $85 + 35 \times 0.5 \times 0.8$, or 99. By the same argument the sibs of whites of IQ 80 should have a mean of 92, but those of negroes of IQ 80, only 83. Jensen claims that these expectations are found in fact; in other words, the data fit genetic predictions. (He does not quote the data, but after correspondence with him I am satisfied that the results are available and do not seem to involve biases such as might have permitted alternative explanations.) Jensen therefore regards the evidence as supporting the genetic hypothesis.

For some time I fell into the same trap. But it is a trap, for populations must regress to their own mean whatever the cause, genetic or environmental, of the mean differences between the populations. This evidence is therefore as compatible with explanation in terms of the environmental factor

X as in terms of the genetic hypothesis. It adds nothing whatsoever to the strength of the genetic hypothesis. All these results reveal is that heritability within the negro group is of the same order as that within the white group.

I do not think this a culpable error on Jensen's part. So much of the argument concerning environmental factors is open to criticism, and he has been subject to such virulent attack (see the documentation in the long preface to his *Genetics and Education*³), that it must be wellnigh impossible for him to criticise the evidence relating to genetic hypotheses as closely and objectively as he has that concerning environmental factors.

Summarised Data

But another example is less pardonable. Jensen, Eysenck and also Shockley among others have each proposed that evidence might be obtainable from inter-group hybrids and from studies of correlations, among 'negroes', between IQ and amount of white ancestry as estimated from marker genes. Jensen is well aware of the difficulties such studies must present, notably with respect to non-randomness of those involved in inter-group matings (see his chapter 9) and he deals in a different chapter with the one piece of evidence of this kind that he seems to think good, giving it much less stress than Eysenck did in his book *Race, Intelligence and Education*⁴. It is, however, a piece of evidence that illustrates well the difficulties and extreme dangers of reliance on summarised data.

This is the evidence of De Lemos⁵ on Australian aborigines, and it concerns Piagetian tests whose results correlate strongly with those of culture-reduced IQ tests. De Lemos gave Piagetian conservation tests to aboriginal children who were divided into two groups, according to Mission records of their ancestry, as full and part aborigines. Most of the part aborigines had, it seems, one white greatgrandparent. Jensen reproduces the data direct from page 316 of De Lemos's paper⁵ (see Table 1), and in this form it looks convincing.

Table 1 Table 17.1 of Jensen's Book¹. (Comparison of the Number of Part-Aboriginal and Full-Aboriginal Children showing Conservation*)

Test	Full Aboriginal N=38	Part Aboriginal N=34	χ^2	P
Quantity	4	18	15.21	<0.001
Weight	16	25	7.23	<0.01
Volume	2	8	3.59	0.05 < P < 0.10
Length	12	20	5.37	<0.05
Area	3	10	4.23	<0.05
Number	3	9	3.22	0.05 < P < 0.10
Total ‡	40	90	36.14	<0.001

* From De Lemos⁵.

‡ The chi square test for Total (given by De Lemos) is statistically inappropriate here, since pooling more than one observation from the same subject violates the requirement of independence of observations upon which the chi square test depends.

But Jensen had already presented these data before⁶ in a different form taken from De Lemos's PhD thesis⁷ as shown in Table 2.

Not only are the significance levels now much less impressive, though total sample size is for some reason rather larger, but one immediately sees that Table 1 confounds an age difference with the ancestry difference, whereas Table 2 shows age to be, as of course it must, most important in relation to these tests. One is left asking whether age differences within the 8 to 11 and within the 12 to 15 year groups might account for the significant results left in Table 2. Examination of De Lemos's original paper shows that age

differences are not adequately controlled, and that the data cannot be regarded as demonstrating that the ancestry difference has significant effects. It turns out that this conclusion has already been published by Vetta⁸, who also refers to a replication study by Dasen, which had negative results. These papers came out too late for Jensen to refer to them, for his bibliography contains nothing later than 1971. But he criticises others in other contexts for failing to equate for age, and he really should have seen the implications of the difference between these two Tables reproduced from his own publications.

I have considered this example in detail because it illustrates the difficulties one is faced with in judging conclusions in this area. Jensen has, often cogently, criticised evidence purporting to support relation between IQ and various environmental factors. Some of his own arguments are open to comparable criticism. The treatment of De Lemos's data and the consideration of regressions suggest that Jensen suffers from the same kind of conscious or unconscious bias as many of his opponents, that is to say he is prepared to accept evidence that seems to support his hypothesis with less critical examination than he would give to evidence purporting to be against him.

Lessons

One is left therefore, despite all the literature, with these conclusions. First, there is no evidence which reveals whether the negro-white IQ difference has any genetic component or any environmental component. Both hypotheses (and any intermediate hypothesis) are equally consistent with the facts. (There is one exception, United States negro women score higher than United States negro men, a difference for which no genetic model seems to fit the facts.

Table 2 Table 1 from Ref. 6. (Numbers of Full-Blood and Part-Blood Australian Aboriginal Children Passing Piagetian Conservation Tests and the Significance Level (*P*) of the Difference*.)

Total <i>N</i> =	Age 8 to 11 years			Age 12 to 15 years		
	Full 25	Part 17	<i>P</i>	Full 17	Part 21	<i>P</i>
Tests						
Quantity	2	6	<0.1	2	15	<0.01
Weight	9	11	<0.1	7	17	<0.01
Volume	0	5	<0.05	2	4	N.S.
Length	10	10	N.S.	3	13	<0.05
Number	0	4	<0.05	3	8	N.S.
Area	1	4	N.S.	2	8	N.S.

* Source: De Lemos⁷.

Jensen himself has elsewhere⁹ put the difference down to environmental factors of the nature of 'X', but does not mention the matter in this book. It should account for at least one tenth of the United States negro-white difference.)

Second, no statement about causation of IQ variation should be taken at its face value, whoever the authority. Every statement requires most careful consideration of the detailed data on which it is based.

Third, the more we would like to believe some statement about the causation of IQ variations, the more closely should we examine the data and logic behind it.

If we learn these lessons, Jensen may have done us a service. He is perfectly correct in maintaining that the hypothesis that group differences in IQ (or anything else) have a genetic component is plausible and that there is no evidence against it. This means that no evidence which purports to demonstrate the relevance of some environmental component can be regarded as adequate unless it be demonstrated from the same evidence that the groups being compared could not differ genetically. It follows also that we have no reason to expect that different groups will have the same distribution of attributes and that demonstrations

that different groups have different frequencies of success, for example in some educational selection process, cannot be regarded as good evidence that that selection process does not provide equality of opportunity for the individuals concerned. These are important lessons that all—including editors—should bear in mind and try to act on.

But I believe there is a risk that Jensen may have also done us a disservice, for the controversy about group differences is proving a stick to beat him with, to the detriment of rational discussion of individual variation and the importance of the genetic component of individual variation in attributes that correlate with IQ.

There is now a danger that the controversy about group differences may lead the evidence about individual differences to be swept under the carpet. But our relatively recent knowledge concerning the ubiquity of genetic variation is of the utmost scientific, political, and philosophical importance. Variation in IQ, and variation in the related aspects of educability, are but one example. Let us remember that, to assume that IQ variation is of no moment, or to assume that it does not have a substantial genetic component, not only flies in the face of the facts, but puts us in a position where it can be held that the state through the educational system may make what it will of anyone. In truth, individuals, however malleable, are different and should not be treated as if they were the same. Controversy about the causation of group differences must not lead us to ignore this, and difficulties that arise because groups differ in the frequency with which they meet certain educational criteria must not lead to our assuming that everyone is able to meet the same educational criteria. It is not true that everyone can reach the same academic standards if provided with adequate opportunity, and the heritability of IQ is a partial measure of that untruth. Equality of educational achievement must prove an unrealisable ideal. Those who raise the hope that it is realisable must bear responsibility for the resulting widespread individual disappointment and all its consequences.

Our societies need to be organised to allow for individual differences, including such differences in aspects of educability. Indeed, if our societies were so organised we might perhaps slowly come to realise that it does not necessarily matter if group differences prove to have a genetic component, for the genetic variation within groups and the overlap between groups are more important and no demonstration that a group difference has a genetic component could justify 'racism'. Perhaps also if we spend less effort considering group differences and more on individual differences and their genetic component we may become more capable of reasonable discussion of the implications of this individuality, the ways in which society should accommodate it, and the extent to which any particular dimension of individuality, such as IQ and its associates, may or may not be overstressed in our present system of status and economic rewards.

I thank Dr J. B. Gibson for drawing my attention to the difference between Tables 1 and 2.

¹ Jensen, A. R., *Educability and Group Differences*, pp. xiii+407 (Methuen, London, June 1973), £3.90.

² Jensen, A. R., *Harvard Educational Review*, 39, 1 (1969).

³ Jensen, A. R., *Genetics and Education* (Methuen, London, 1972).

⁴ Eysenck, H. J., *Race, Intelligence and Education* (Temple-Smith, London, 1971).

⁵ De Lemos, M. M., *Int. J. Psychol.*, 4, 255 (1969).

⁶ Jensen, A. R., *Harvard Education Review Reprint Series*, No. 2, 211 (1969).

⁷ De Lemos, M. M., thesis Australian National Univ. (1966).

⁸ Vetta, A., *Int. J. Psychol.*, 7, 247 (1972).

⁹ Jensen, A. R., in *Contributions to Intelligence* (edit. by Cancro, R.) (Grune and Stratton, New York, 1971).

Book Review Supplement

GENERAL BOOKS

Simulating the Obvious

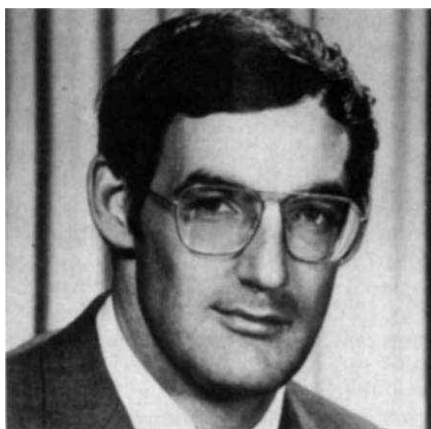
Toward Global Equilibrium: Collected Papers. Edited by Dennis L. Meadows and Donella H. Meadows. Pp. x+358. (Wright-Allen: Cambridge, Massachusetts, April 1973.) £9.

THE work of Meadows and his colleagues, sponsored by the Club of Rome and published in *The Limits to Growth*, has been so riddled with criticism (for example from the World Bank and from a group of workers in the University of Sussex) that one opens another book by these authors with doubts about their judgment, and scepticism about the value of systems analysis for enquiry into global social problems. *Toward Global Equilibrium* is a volume of collected papers by Meadows and his co-workers, in which the now familiar technique of computer simulation is applied to seven specific problems: the movement of DDT, mercury contamination, eutrophication of lakes, the utilization of a natural resource, solid waste generation, a case study of natural gas in the USA, and population control mechanisms in a primitive agricultural society.

These seven studies do, at any rate, counter one criticism of Meadows's earlier publications: the criticism that it is grossly misleading to put data into a computer for such vague generalities as aggregated global pollution or aggregated global availability of natural resources, and to draw any conclusions at all from the simulations. Several critics suggested that if the technique were applied to much more modest problems it might produce interesting results. That is what has been done in the studies described in this book.

The simulations for DDT are made on the assumption that its use continues to increase, or remains at its 1971 level, or is phased out by 2000, or is completely banned from 1971. The results show that even with a ban from 1971, the DDT level in fish will still be perceptible in the year 2020. The simulations for eutrophication show how the application of algicide would affect the oxygen content of a lake. The simulations for solid waste generation describe what happens when assumptions are fed into the computer about incentives for recycling, taxes on extraction from

raw material, or altering the amount of raw material in the product. The simulations for the "life cycle" of natural gas in the USA draw what its author describes as a "fundamental conclusion", namely that a finite resource has a "behaviour mode", namely "an initial period of unrestricted growth in supply, a transition period when growth is slowed, and finally a decline in supply".



Dennis Meadows

The question one is bound to ask is whether it needs a computer program and sixteen print-outs to reach this fundamental conclusion. Of course a point will come when the cost of exploration for new sources of supply exceeds the profits likely to be got from selling the product. Of course, if the price is not pegged, the cost may rise so high that consumption falls. But do these simple relationships need to be displayed as the unexpected disclosures of systems analysis? This is the question one asks in practically every essay in the book. It is common knowledge that DDT persists in the environment and that much of it is being transferred from land to the oceans. Therefore it does not need the sophisticated analysis in this book to reach the conclusion (on page 74) that there will be a lag between the curve of declining usage-rate of DDT and declining levels of DDT in fish. Nor does it need systems analysis to persuade anyone that if the price of a natural resource is regulated this may affect investment in exploration for more of the natural resources (page 239). On page 199 there is a discussion

on the effects on waste-generation of reducing the amount of raw material per product. If this makes the product cheaper and if the market is not saturated, the effect may well be an increase in the amount of waste-generation because more people buy the product. This is an interesting observation, and it is not made more credible by titivating it in a computer; but this is described by the authors (on page 200) as the "kind of behaviour that is in fact the *raison d'être* for the system dynamics approach".

In brief, it is the text of this book which is in places illuminating, not the diagrams of computer-runs. The authors are sincerely concerned about the conflict between short-term and long-term goals for industrial societies. They are right when they say that politics are heavily biased toward the achievement of short-term goals. They are right when they say that a "mismatch exists between the time span of development of environmental problems and the time horizons of institutions designed to deal with those problems". One puts this book down with gratitude to the authors that they have alerted millions of people to something which every biology student knows—that in nature all exponential curves become sigmoid—and with disappointment that the authors have conveyed this important truth by a meretricious technique which may cause a backlash of public opinion.

ERIC ASHBY

Mexican Sculpture

Chalcacingo. By Carlo T. E. Gay. Pp. 122 (24 plates). (Akademische Druck- und Verlagsanstalt: Graz, 1971.) öS 700.

To anyone who has studied the numerous small carvings of jade, serpentine, and other hard stones that have been discovered through the years in Guerrero, Mexico, the existence of a vital art tradition has been obvious. While many of these are clearly Mezcala in style, others have equally well defined characteristics of that more generalised style called Olmec. Figures, animals, small masks, ear plugs, "pectorals", pendants, or other more abstract shapes, commonly

possess those qualities of strong form and careful craftsmanship typical of a well developed and highly sensitive culture.

Because of the great aesthetic quality of this sculpture as well as the relative abundance of it in the area of the Balsas River, it has been difficult to believe that it was the "provincial" product of a culture whose "heartland" was really the Gulf Coast sites of San Lorenzo and La Venta. Such scepticism could only be reinforced by a visit to the carvings cut in living rock of the Cerro de la Cantera at Chalcacingo in nearby Morelos.

Carlo T. E. Gay's book on Chalcacingo is not only the first systematic study of this important ceremonial centre but is also a most coercive presentation of the various pieces of evidence suggesting that the central highlands were the site of a formative, rather than a provincial, phase of Olmec development. While his arguments for such a position are based on a deductive analysis of what is essentially surface material, they are convincing enough to stand until the completion of far more extensive and systematic excavation than has thus far taken place in this area.

While some of Gay's evidence has been known and published before, he has discovered much of it himself in the past few years and is publishing now for the first time. Fundamentally, it falls into three distinct parts: the rock carvings and altars at Chalcacingo, the wall paintings deep inside the Juxtlahuaca Cave near Colotlipa and, perhaps most impressive, the pictographs located in small niches and rock shelters in the Cerro adjacent to the Chalcacingo carvings.

The elemental quality of these pictographs, their obvious pairing and placement within an intelligible context, and their location on the face of the Cerro leave little doubt of their importance to the ancient people of the area. Gay carefully analyses each pictograph and attempts to compare and relate it to similar signs found elsewhere in Guerrero or to the palaeolithic symbols known in western Europe. Utilising the precise drawings made by Frances Pratt, he provides a more detailed description of the pictographs and their organisation than can be easily determined on the site. In fact, his book is an indispensable guide to anyone wishing to examine these incredible paintings at first hand. His dating of the pictographs at the middle of the second millennium BC seems somewhat cautious in terms of the rock carvings adjacent to them but, as he is well aware, precise dating of the entire Chalcacingo complex will be possible only with the evidence derived from more field work.

If the pictographs are the most fas-

cinating, the ten rock carvings, the subjects of which include highly naturalistic representations of plant, animal and human forms, as well as symbolic and obviously composite images, are the most truly amazing aspects of Chalcacingo. Although they vary in scale, the major carvings range in height from about four to a maximum of nine feet, and the largest extends to twelve feet end to end. All are shallow relief carvings with the images projecting from the background. Again with the help of Miss Pratt's superb drawings, Gay describes each carving and analyses its iconographic elements in relation to other aspects of Olmec art. He sees close parallels with the Juxtlahuaca Cave paintings, as well as the earlier pictographs at Chalcacingo, and attempts to define the symbolic elements common to all of them. Their close association with a group of more than twenty altars carved in boulders and in bedrock adjacent to the reliefs suggests that the site was used for extensive propitiatory ceremonies. Presumably a development later than either the pictographs or the cave paintings, the carvings cannot be dated with any precision at present. The author suggests, however, that they were probably carved before the end of the second millennium BC.

In addition to providing the reader with a thorough survey of surface material to be seen at Chalcacingo, Dr Gay has made three important deductions which merit serious consideration. Perhaps most significant is his suggestion that the beginnings of the Olmec tradition are to be found in western Mexico rather than on the Gulf Coast. Furthermore, he suggests that the basis of the organisation of both the pictographs at Chalcacingo and the paintings in the Juxtlahuaca Cave bears close similarities to related material in palaeolithic Europe. Without drawing a precise conclusion from this phenomenon, Gay opens a line of inquiry which he and others may soon follow.

His second deduction which is almost equally challenging is that the serpent rather than the jaguar was the central animal motif in Olmec religion and iconography. The composite forms of human and animal features so common to Olmec art then become the combination of man and serpent rather than man and jaguar. The third deduction of importance, again drawn essentially from the carvings at Chalcacingo, is the pre-eminent position of the rain god in Olmec religion. The principal relief would certainly seem to be a propitiatory image with abundant rain or water symbols, and another relief, at the very top of the Cerro, is interpreted by Gay as "the rain deity, dwelling in the high spheres, whose magic hand extends toward the clouds to make rain fall on the fields below."

A minor omission, which might be felt by someone using the book as a guide to Chalcacingo, can be mentioned. One of the most impressive and provocative experiences to be had by a visitor to Chalcacingo is to arrive at the site late on a summer morning. Shortly before noon the Sun moves into a position that casts a perfect raking light across the principal carving. This, of course, brings out all the fine detail and the carving comes alive as the astounding work of art it is. As the time passes and the Sun moves, it illuminates other reliefs in turn, giving each of them a clarity of definition not seen otherwise. While the author has noted this phenomenon in a previous article on Chalcacingo, mention of it might well have been included here.

If there is any major shortcoming of Gay's book, it is that common to any first study; it seems to raise more questions than it answers. In the field of Middle American archaeology and particularly in the area of Olmec studies, this may at this time be more of a virtue than a failing. Theories have been built on too little evidence and too little looking. Carlo Gay has looked much, thought a lot, and tried to bring insight to an area where little is known.

MERRILL C. RUEPPEL

Indian Science

A Concise History of Science in India. Edited by D. M. Bose, S. N. Sen and B. V. Subbarayappa. Pp. xvii + 689. (Indian National Science Academy: New Delhi, 1972.) Rs.50; £6; \$16.

THIS substantial work, a full two inches thick in its hard-back covers, is certainly the most useful and reliable compendium on the history of science and technology in Indian culture which has yet appeared. Published for the "National Commission for the Compilation of History of Sciences in India" by the Indian National Academy of Science, it necessarily has a quasi-official character, but since it is a collective work it must be taken as the collaboration of some dozen or more scholars, whose results naturally diverge at times in details. The academy is to be warmly congratulated on the success of the enterprise, with D. M. Bose as editor-in-chief.

Each of the two editors has made himself responsible for one or more chapters of the text. Thus Samarendranath Sen writes on both mathematics and astronomy; but also contributes on the botany of the Vedic and post-Vedic ages. Similarly, B. V. Subbarayappa deals with alchemy and early chemistry, concepts of the physical world, and the coming of modern science; but adds his help to the chapter on agriculture. They are joined by R. C. Majumdar for medicine, while the sections on botany, zoo-

logy and agriculture are the work of groups led by K. A. Chowdhury, J. L. Bhaduri and S. P. Raychaudhuri respectively. Broadly speaking, the work concentrates, quite reasonably, on the Sanskrit and Tamil literatures and traditions, but the Arabic and Persian influences are not neglected, for M. Z. Siddiqi writes on the Unani-Tibb medical system (Hellenistic-Arabic), and K. A. Chowdhury on the mediaeval botany of Urdu culture.

The general structure of the work is as follows. It opens with a chapter entitled "Survey of Source Materials" by S. N. Sen, a contribution of capital importance, since the history of science and technology in India has long been bedevilled by the great difficulty of dating ancient and mediaeval texts. Even now they can often only be fixed to within a century or so, but some progress in this fundamental philological endeavour has been made, and the introduction seems fully objective, giving adequate warning of differences of opinion where they exist. Armed with this yardstick the reader can fearlessly follow the succeeding eight chapters which deal with the sciences and technologies topic by topic. At the end comes a résumé chapter, by B. V. Subbarayappa; this is nearly fifty pages long, including a chronological table, and summarizes the results of the previous 567 pages.

A research tool of this magnitude has to be judged in part by its apparatus, and the present volume measures up to that ideal quite well. There are tables of standard abbreviations for journal references quoted, and, more interestingly, for the titles of the original primary sources in Sanskrit, Tamil, Arabic and Persian. A good bibliography of these follows, with indications of translations into Western languages if any such exist. Here it would have been rather helpful to have given a brief dating in each case, or at least a cross-reference to the page in the introductory chapter where that dating is discussed. After this comes the bibliography of secondary sources, sufficient up to a point, but with some curious deficiencies; for example, (a) one would have expected more than just a single reference to the outstanding work of David Pingree on Indian astronomy, (b) the name of Abdul Rahman does not appear at all, though one might well have expected to find him among the collaborators for the Arabic-Persian sector, (c) S. Mahdihassan, repetitive but occasionally inspired, is omitted from the bibliography, and (d) apart from Mikami Yoshio's old work on East Asian mathematics there seems to have been no use of modern Japanese studies of Indian science such as those of Yano Michio. Lastly, there is an index of twenty-eight pages; had it been twice as long, such a fine work as this

would have been twice as useful. Entries of major importance are missing, and an entry with but one page ought to have had a dozen or so. For example, there should have been such entries as follows (I add the pages since readers may like to note them): alloys (304, 308), *amṛta* (285), brass (299), bronze (304, 306), distillation (306, 333), elements, five, see *pañcabhūta*, flux (302), fuller's earth (337), "intentional language" (338), stirrups (611), and so on and so on.

In general, the book is highly factual, with abundant provision of terminology and nomenclature, and there is little speculation. When this does come in, it is mostly in connexion with relations with other cultures, with influences and transmissions. In mathematics and astronomy the questions of interchange with the Hellenistic world and China are naturally prominent; in alchemy Chinese and Arabic culture are to the fore, and here the interesting suggestion is made (page 589) that the introduction of salt by Paracelsus into his *tria prima* could perhaps have been influenced indirectly by the earlier salt-based *muppu* alchemy of Tamilnadu, one exponent of which, Ramadevar, is known to have visited the Arabic culture area and taught the doctrine there. But on the whole this notable handbook will be used primarily for its factual content, and though it does not replace earlier guides such as the valued work of Renou and Filliozat, it is wholly devoted to science as theirs was not, and it will be an indispensable addition to the armamentarium of all scholars interested in this noble subject.

JOSEPH NEEDHAM

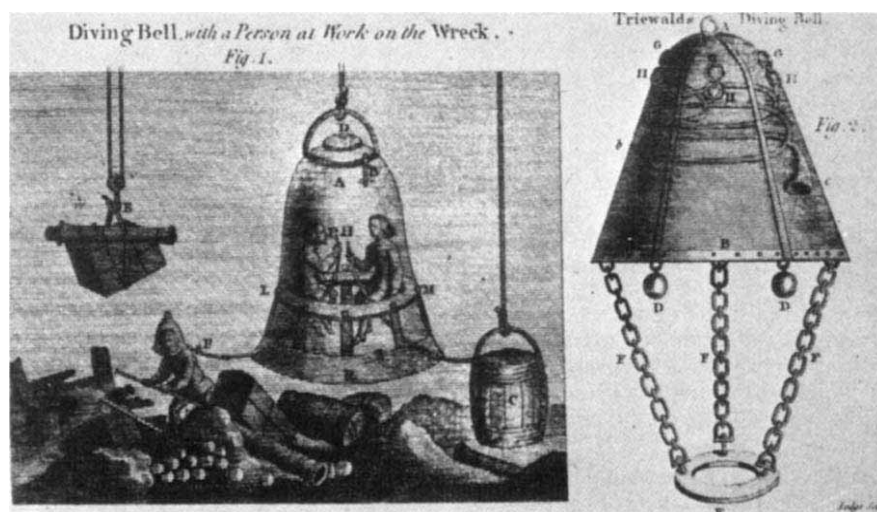
Pedigrees of Words

Pedigree: Essays on the Etymology of Words from Nature. (The New Naturalist.) By Stephen Potter and Laurens Sargent. Pp. 322. (Collins: London, September 1973.) £3.25.

JUSTIFIABLY, the Oxford English Dictionary (OED) usually stops when it has traced a word to a source or root in one of the innumerable languages that we have raided in constructing our language. At that point the lexicographer feels that his job has been done. The OED sometimes unbends a little and, in small type, discusses the source. Potter and Sargent excel in this supplementary role. They concentrate on biological words; especially on the names of birds. For about half the words they do not add anything to what is in the OED. But they present the material in a handy form, and add colour and many anecdotes. Both sources agree, for example, that pedigree comes from *pied de grue*, because a crane's foot resembles a mark used in genealogical tables; that grow, green and grass have a common origin; that groundsel has an intrusive *r* and was originally *gund* (=pus) swallower; and that "to curry favour" was originally "to curry favel"—Favel was the name of an imposing horse in a fourteenth century French story.

Going beyond the OED, they relate hybrid with hubris: the combination "hybrid vigour" is therefore tautological. They derive elephant from its Sanskrit name *ibha* and the Semitic article *el*. They derive dupe from the French for a hoopoe—a supposedly stupid bird, so that anyone behaving in

Diving Device



Diving bell invented by the astronomer Edmund Halley in 1720. Divers received a continuous air supply through tubes attached to a cask lowered from the surface. From the chapter on the early history of diving by Robert E. Marx in *Oceanography*, edited by R. Gordon Pirie (Oxford University: London, New York and Toronto, July 1973; boards £5.25; paper covers £2.70).

the manner *d'huppe* was a dupe. A very interesting derivation is *os*=face, hence *oscillum*=little face: the masks of Bacchus, hung in vineyards as scarecrows, oscillated.

For all their erudition, they admit defeat over a few words, for example drake; they say little about some others, and omit a few interesting ones altogether. Thus ladybird gets three lines—you could fill twice that space with its aliases. Why did that insect excite our inventive faculties so greatly? Nostoc is not mentioned. According to the Authorised Version the Israelites said, "It is manna": for they wist not what it was". A marginal note says that manna=Hebrew *man hu*=what is it. Paracelsus probably turned that into dog-latin and contracted (as was his habit) getting *noste hoc* and so nostoc, for he thought that was what they ate. Potter and Sargent follow the OED into error in deriving Jerusalem artichoke from *girasole*. Salaman argued that the etymon was the Netherlands town Ter Neusen. They comment on the layman's preoccupation with hares in naming plants, but not on the systematist's preoccupation with wolves: for instance, *lycopodium*=wolf's foot, *lycoperdon*=wolf's fart, and *lycopersicum*=Persian wolf. Why is the last a suitable name for the tomato?

As well as discussing derivations, they discuss linguistic trends. Transposition, or metathesis, is common in all languages. In English, *bridd* became bird, *hundert* hundred and *wops* wasp. This happened to carmine (from the kermes insect) when it became crimson while passing through the Near East. The original root *ter*=to rub is still there in termite and teredo, but metathesised into trypsin and trypanosome. They comment on the tendency of early forms of language to stress the actions rather than the appearance of animals, and to use names to relate organisms on the basis of rather slender resemblances, whereas we use names divisively. On the basis of the first trend they comment on the paucity of names for colours in Greek, and say that Phryne was so called because of her golden-brown skin. Nowadays, most whores would resent being called toad regardless of their colouring. There is a real difficulty here. The Greeks painted their statues, and that group of our contemporaries, which we arrogantly call primitive, often have an enthusiasm for colour that surpasses our own. Perhaps initially you just use colour and do not waste time talking about it.

Although they comment on some interesting inconsistencies in the use of plurals they do not mention beans, oats and peas. These are consistently plural, presumably because they appeared on the table as individual pieces, like turnips and potatoes, unlike

flour from such cereals as wheat. They comment on the old double plural kine, for the i (originally y) has already made cow plural, and on the use of horse in both singular and plural. Apropos of horse: the sudden replacement of *equus* by *caballus* is an old puzzle. A philological wit suggested that the new word was part of an erotic joke, few things spread through a community so fast. The suggestion here is that *caballus*, the root of most horse-words in Romance languages, has a Celtic origin.

It is clear from this excellent book that spelling used to be fluid but meaning fairly rigid. Artistry put the intrusive b into limb and thumb, and pseudo-classicism the h into ailanthus, the p into ptarmigan and the ridiculous ph into sulfur. Since Dr Johnson, at whom fun is from time to time poked, spelling has been rigid and meaning fluid. Misuse is so common that a careful writer must avoid many words. Americans have invented new meanings for career and watershed. On both sides of the Atlantic enormity and vicarious are losing their meaning. So long as it is spelt with z, if that is the "house style", modern editors allow visualise to mean "to make visible". We should heed T. S. Eliot's warning:

Words strain,
Crack and sometimes break, under the
burden,
Under the tension, slip, slide, perish,
Decay with imprecision, will not stay in
place,
Will not stay still.

N. W. PIRIE

No Faith in Figures

The Numbers Game: The Bland Totalitarianism. By Harry Hopkins. Pp. 310. (Secker and Warburg: London, March 1973.) £2.80.

THIS is a view-with-alarm book. The "numbers game" so viewed consists essentially of doing one or more of the following misdeeds: counting or measuring things that should not be quantified; doing so most inaccurately; trumpeting, or brooding over, or acting upon those figures to a degree unwarranted by their worth. Here is the world our author views.

The stock market plunges on announcement of a preliminary estimate of a balance-of-trade figure. Such is the power of a number carried to two decimal places that no one stops to ask how precise such an estimate can be, or what produced the change from the preceding period, or even just what the quantity being measured represents anyway.

Or a teacher acts on the evidence of a single magic number—because no verbal description of a child, or subjective knowledge from personal acquaintance, has the hypnotic quality

of an officially entered IQ 113 in his record.

Through sampling methods, every hour of every major American television programme is "Hooperated". This guarantees that if detectives and horror figures are popular at the moment, there will shortly be nothing else on the air.

Scientific method falls upon British Rail. Measurement and calculation reveal that one third of the route mileage is carrying a mere 1% of the traffic and inevitably doing so at a loss. The all-too-obvious sequel threatens the comfort, convenience, and possibly the livelihoods of no negligible number of persons, because even 1% of passenger journeys is 9,384,330 journeys.

And what of the magical GNP? A drop, not in the gross national product but merely in the rate of its growth as measured by a mysterious index, is enough to precipitate financial panic. Yet the GNP figure is so incomplete that it can show a drop when there has in fact been a rise in true production. (If I pay a typesetter \$100 to improve my bathroom, that amount finds its way into the GNP. But if I tile the room myself, adding equally to what has really been produced, not a cent is added to the index. The tile might as well lie unused in the box.)

In pointing out these abuses, Mr Hopkins is on the soundest of ground. We do talk too much in numbers. We do use figures that are more or less irrelevant or badly put together. But not all the abuses our author parades for us really have to do with numbers. It may well be that substitution of blood purchase in America for a former system of reliance upon charitable-minded (and healthy) donors has increased the prevalence of hepatitis among transfused patients. But it is stretching terms a bit to call commercialization, however deplorable, a numbers game.

Since Mr Hopkins is not entirely prepared to abandon index numbers and sampling studies and censuses (he does not hesitate to use them in pursuit of his own ends here) and the other methods in which he finds abuses, we should like him to tell us where to draw lines. Which numbers should we abandon? How can we persuade publicists to stop shouting at us the figures that favour their particular causes? How are we to keep casual readers from taking at face value headlined numbers that do not, in fact, mean much of anything?

Mr Hopkins does not give us much help at this point, though he does suggest that a little schoolroom practice by children in manipulating statistics might meet their need to be "inoculated against numerical neuroses as well as against smallpox and diphtheria"; to which almost anyone, particularly after reading *The Numbers Game*, would say amen.

DARRELL HUFF

Atomic Politics

Nuclear Politics. By Andrew J. Pierre. Pp. xiii+378. (Oxford University: London and New York, 1972.) £5.50.

THIS book is most useful and perceptive. Before very long, the next stage of Britain's nuclear weaponry will have to be decided. This is a subject which is inseparable from its history, yet that history lies in the twilight, "contemporary" zone of the last thirty years which is especially obscure to young, even middle-aged, adults of today. The possession of a nuclear weapons system by Britain, a rather small country and a low economic performer, seems a curious aberration. How did it come about? Who can now remember what exactly Blue Streak or Skybolt was or what happened at Nassau, or what the initials ANF or MLF mean? (For those who do forget they stand for Atlantic Nuclear Force and Multilateral Nuclear Force; when the former torpedoed the latter, it was said to be the first case in history when a non-existent fleet sank another non-existent fleet.) In the absence of serious histories of the whole business, putting all the stages in perspective with one another, popular conceptions and official policy making alike tend to be based on myth.

There are of course great difficulties in writing histories of such events without access to the official documents, which are not yet openly available. Interviews with leading figures can be more misleading than helpful unless personal testimony can be checked against the records. Nevertheless, in periods where there is much public

debate and controversy on a subject, a knowledgeable and conscientious historian can probably get near the truth. Thus Dr Pierre's part three, "The Great Nuclear Debate 1957-64", is particularly good in narrative and analysis. This part together with his fifth part—Conclusions—make the book valuable. The first part, "The Wartime Genesis", is largely a summary of official history. Part four, "The Dénouement 1964-70", and part two, "Towards the 'Independent Nuclear Deterrent' 1945-57", are less satisfactory because there was much less public controversy.

Atomic policy in the early postwar years was shrouded in especially intense secrecy, and Pierre is therefore inevitably circumscribed in his sources. The Government has just authorized the publication of the second instalment of my official history of the British atomic project for the period 1945-1952, and I am convinced that the subsequent history of Britain's nuclear policy cannot be fully understood without a clear picture of this formative period; in it atomic decision making was most confused, yet the scientific and technological execution of the project was excellent in terms of performance and cost. (Pierre's figures for the cost of Harwell on page 121 are 1,000 times too high.) The independent deterrent was embodied in British policy earlier than Pierre suggests, although the advent of the hydrogen bomb defined the policy more sharply in the mid 1950s. Above all, this period was marked by harrowing, unsuccessful negotiations with the United States. Even though in the mid and late 1950s the Americans restored generous atomic collaboration, these early postwar years left their trauma. Resolve to maintain independent nuclear weapons had become a matter not only of prestige and status but of basic self respect. So the mistrust of the United States which Pierre emphasizes had become deeply rooted. Pierre quotes R. H. S. Crossman as saying in 1958 "the right to distrust the Americans cannot remain a British monopoly", but no other country had had such unhappy experiences. The Anglo-American negotiations of this period, moreover, prevented any collaboration with Europe, including France, even though the British would have done no wartime work on nuclear reactors if French scientists had not come over with the world stock of heavy water in 1940. The French did not forget their treatment in the early postwar years. Nuclear collaboration with Commonwealth countries, other than Canada, was also ruled out. (Pierre leaves out this connexion almost entirely.)

Nevertheless, Pierre's general feel for the motivations of British policy and for its dilemmas is fair and shrewd,

sympathetic as well as critical; he is conscious of the need to keep options open "in a highly unpredictable world". Pierre brings the scientists only into the wartime story, although he lists their interests, pressures and perspectives among those which shaped the British nuclear force. (He does not add engineers to this list.) Yet he is conscious that if the British scientists in 1940 (including the refugees) had not seen, more clearly than those in any other country, the feasibility of the atomic bomb, the whole story of British nuclear weapons might have been very different.

MARGARET GOWING

Rivett and the CSIR

David Rivett: Fighter for Australian Science. By Rohan Rivett. Pp. xii+225+23 photographs. (Dominion: North Blackburn, Victoria, 1973.) \$5.95.

BIOGRAPHY is always most difficult to write: even more so when, as in this case, the author is a son, concerned not with exposing intimate relationships, that is the familial and social life-blood of the human being, but with presenting an "objective" description of the main events of a famous father's life. As an introduction to a remarkable personality, this book will serve. I await with pleasure Marjory O'Dea's "poetic" life of David Rivett.

This book is more interesting as an example of the development of first generation science policy. The story is familiar: it is typical of the way in which science came to be used by the state in all industrialized countries between the two world wars.

First, the politician is made aware that science can help to develop the country's natural resources, and thereby improve productivity and raise living standards. Second, he seeks out a scientist to put in charge of a body responsible for harnessing science in the nation's interest. Third, funding is minimal. Fourth, suspicions about the new body are everywhere—from scientist to local politician. Fifth, socially and politically a stage is reached finally at which the pioneers can no longer cope with the qualitative changes. It is time for the next generation of science policy, and for the fading out, with suitable honours, of the progenitors.

Australian Prime Minister S. M. Bruce was fortunate in his choice of chemist David Rivett, outstanding son of an outstanding, forthright Protestant minister, as chief executive officer, and in harnessing him with successful engineer, Sir George Julius, as chairman of the new body. Of course, the new body was particularly fortunate in having a Prime Minister interested in

Bomber Fly



Chinese poster of 1937 warning of the dangerous diseases carried by flies, in *Flies and Disease* (Volume 2), by Bernard Greenberg (Princeton University: Princeton, August 1973, \$18.00).

its development; else, in the sparse atmosphere of 1926 when the Commonwealth Council for Scientific and Industrial Research (CSIR) was born, it would not have survived.

Rivett was a puritan, fanatical, single-minded. Without him, there would have been no highly successful CSIR. He did not spare himself. In the days before air travel was a commonplace, he spent scores of uncomfortable days in trains getting to know local conditions and people. His family life was minimal; yet there was an essential solidity and sincerity of affection with his wife and children without which he could not have survived.

What were his achievements? Without doubt, to have created an atmosphere and an organization in which science in the service of the nation—and funded by Federal Government—could take roots. He preferred Australians with experience abroad, to whom he gave complete support in their research—at the expense of a comfortable HQ office. This high regard for the man at the bench, and his lesser attitude to the administrator, never changed. It was well expressed in his briefing to a plant pathologist, B. T. Dickson, who agreed to leave McGill University in 1927 to become CSIR's chief mycologist and second chief of a division. He explained there was an abundance of problems and practically no organization. "This Council has been in existence for only a year and to a very great extent its plans have yet to be made. But briefly the object at which we are aiming is to get together a band of enthusiastic leaders in different branches of science (chiefly those associated with the primary industries) and to afford them every opportunity for initiating and developing organizations for attack upon national problems."

The Second World War made it urgent to concentrate scientific effort in the secondary industrial field, to concentrate on specific problems. Rivett aided in this, of course. But he felt that CSIR—as he wrote in 1946—was turning "into a mob of testers for industry and I hate it." He did not want to handle the routine everyday problems of industry. He was not against the building up of secondary industries, but he believed passionately that only through basic research would come real strength.

He succeeded Julius as chairman of CSIR, but the postwar "cold war" years with the emphasis on security made him unhappy. He did not accept that secrecy and integrity in science could flourish together, and he resigned in 1949 when the CSIR was placed under the public service, at a time when charges of being weak on security and of allowing Communists to work on security projects were made and debated in the House of Representatives.

When W. L. Morrison, the Minister for Science, dissolved the Advisory Committee on Science and Technology last February, he insisted that science and technology were no longer polarized, and that there should be no rigid divisions between the natural and the social sciences. He was announcing—although he may not have understood he was—a new generation of science policy, which would have been impossible without the sound foundations laid by David Rivett.

MAURICE GOLDSMITH

Leibniz

Leibniz and Dynamics: The Texts of 1692. By Pierre Costabel. Translated by Dr R. E. W. Maddison. Pp. 141. (Hermann: Paris; Methuen: London; Cornell University Press: New York; 1973.) £3.

In 1960 Pierre Costabel published two previously unknown texts by Leibniz on mechanics, adding a commentary and an account of their origin amounting in all to a short book. The subject of the present review is a recent translation of this work into English. The first of the Leibniz texts, the "Essay de Dynamique", has been provided with a translation; the second, entitled "Règle générale de la Composition des mouvements", is given, for no clear reason, only in the original French.

Costabel's work comes struggling towards the reader through a translation of very poor quality, in which infelicities of style and vocabulary abound, and downright mistakes are not infrequent. We hear of being "unaware a complexity", of something's being unescapable, of a battle's being waged, of an "amazing perpetual secretary", of someone "urged by the collector's passion". Someone's doubts are "raised" when what is meant is that they are assuaged; the English version of Definition 1 of the *Essay on Dynamics* does not make sense (though fortunately in this case one has the perfectly clear French version on the facing page). This sort of thing tends to sap one's confidence—one begins to wonder just how much of what one is reading is actually due to the author; or for that matter, to the translator, for stretches of the book are so clumsy that one suspects that some unnamed non-English editor must be partially responsible for the outcome.

The central theme is the controversy between Leibniz, on the one hand, and the Cartesian scientists of the Paris Academy of Sciences on the other, concerning the nature of the principles of conservation to be used in mechanics. Chapter 1, rather pretentiously called "From the History of a Discovery to the Discovery of History", introduces this controversy as background to the two texts, and tells of their discovery and

the identification of their copyist. One of the difficulties I had with the book is to do with the fact that the exact nature of the debate is rather slow to emerge. The Cartesians believe in the conservation of "force", measured as "quantity of motion"; and as the latter is the product of mass and velocity it seems that they are asserting, and Leibniz denying, a principle of conservation of momentum. But one discovers (and this is, I feel, the sort of thing which a good exposition would make clear much earlier) that it is Leibniz who is asserting this principle, since it turns out that the Cartesians regard "velocity" as scalar, whereas Leibniz takes it to be a vector. Again, I can't help thinking that everything would be much more readily comprehensible to the modern reader if "energy" had been used for "force" in most contexts throughout the book.

Problems of comprehension held me up many times during the reading of this book. They may be the product of the author's far greater familiarity with the subject matter, but they are nevertheless a defect. I do not think that the interested layman is going to enjoy *Leibniz and Dynamics* very much. Still less, at £3, is he going to wish to buy it. The professional historian of physics will be grateful to Fr. Costabel for the discovery and publication of the manuscripts. But he may well feel that the task of unmistakably charting the course of the controversy to which they contribute is one that remains to be done.

E. J. CRAIG

Tortured Tree



One of the rare Tortuosa beeches, whose malformations may have been caused by radioactivity from meteors, is illustrated in *The International Book of Trees* by Hugh Johnson (Beazley: London, October 1973, £9.95).

PHYSICAL SCIENCES

Magnetism

Magnetic Interactions in Solids. By H. J. Zeiger and G. W. Pratt. (The International Series of Monographs on Physics.) Pp. xv+660. (Clarendon Press: Oxford; Oxford University Press: London, June 1973.) £19.50.

MAGNETISM is a fundamental part of most topics in solid state physics. It is usually recognized in the form of ferro-, ferri- or antiferromagnetism, but probably less easily so in the case of spin-waves or domain structures. Such bulk properties describe ordered magnetic systems but any real explanation of these phenomena must ultimately depend upon the electrons and nuclei of the system and on their interactions with each other and with any externally applied perturbations. The authors' aim in writing this book is to develop systematically the underlying theory of such interactions, emphasizing particularly the physical interpretation of the theory rather than using a rigorous mathematical formulation.

The first chapter introduces the magnetic Hamiltonian. It is used to describe the interaction of an electron with external electromagnetic and uniform magnetic fields, with the atomic nucleus and with other electrons. The orbital and spin contributions are distinguished and relativistic corrections are considered. The next chapter describes the magnetic properties of one-electron atoms. The effects of spin-orbit coupling and the Zeeman interaction are discussed in detail and some of the important terminology is introduced at this stage. The next step is to derive the magnetic properties of many-electron atoms, and this forms the basis of the next chapter. Some of the fundamental principles of indistinguishability are discussed first of all and used in the setting-up of effective one-electron Hamiltonians. The Hartree-Fock approximation is then considered and the L-S and j-j coupling schemes are explained. Calculations of the coulomb and spin-orbit interactions are given in detail and the general magnetic properties of atoms are discussed.

The detailed chapter on the magnetic properties of ions in crystal fields is the longest in the book. The approach is not conventional in so far as the crystal field approach is described by considering in detail the examples of Cu^{2+} and Ni^{2+} ions in octahedral surroundings, while the rare-earth ions are mentioned only briefly. The concept of the spin Hamiltonian is introduced and covalency effects are treated in some detail. There are two further long sections in this chapter—one discusses the Jahn-Teller effect and the other

considers exchange interactions in insulating crystals.

Having discussed at considerable length the properties of systems in which the electrons are localised around particular defect ions in a solid, the other extreme approximation in which the electrons are completely free is then analysed. The one-electron band approximation is introduced and the paramagnetism of the conduction electrons is calculated. The properties of electrons when a magnetic field is applied are then considered in detail—the eigenstates are calculated, cyclotron resonance examined and the interaction with acoustic waves is described. The de-Haas-van-Alphen effect is then described and its importance in determining the Fermi surface in metals is emphasised. This is followed by a discussion of the energy of electrons coupled to lattice vibrations in the presence of a magnetic field and by conduction electron spin resonance and nuclear magnetic resonance in metals.

The next chapter discusses the $\mathbf{P}\cdot\boldsymbol{\pi}$ perturbation theory and the effective mass approximation (where $\mathbf{P}$ is defined as $\mathbf{p} + (e/c)\mathbf{A}$ and $\boldsymbol{\pi} = \mathbf{p}/m$) particularly for the case of band electrons in a magnetic field. The effective mass concept for the electron in many situations is discussed. This is followed by an analysis of interband and intraband transitions and the effect on transitions by an externally applied electric field. Faraday rotation and the magneto-optic Kerr effect are then considered.

Magnetic interactions in semiconductors and metals are the subjects of the last two chapters. Donor and acceptor states for Si and Ge are considered in detail and their properties when a magnetic field is applied are analysed. It is shown how e.s.r. and e.n.d.o.r. can be observed in donor and acceptor states. The concept of an exciton is also introduced. In the case of metals, the existence of localised moments is examined and their effect on the conduction properties is discussed. Then it is shown how indirect exchange interactions and hyperfine effects can also be observed in metals. A ninety-page appendix on assorted topics is included at the end of the book and provides further very valuable background material.

This book, then, covers in considerable detail the fundamental problems of magnetic interactions in solids and would appear to be one of the most comprehensive texts on the subject. It could be argued that it is not complete as the fundamental ideas have not been applied to explain the properties of ordered magnetic systems. The authors have quite clearly decided, however,

that the more fundamental approach is preferable and have left it to others to apply their ideas to the bulk properties of magnetic materials.

The material is presented in a logical sequence and in a self-consistent manner; but for a book of this size, relatively few references have been given. In particular, in the very long section on crystal fields, I am very surprised that no reference or use has been made of the basic crystal field calculations of Hutchings or the tensor operators listed by Smith and Thornley. Also the subject matter of this chapter has considerable overlap with *Electron Paramagnetic Resonance of Transition Ions* by Abragam and Bleaney. This is somewhat surprising as both books are in the *International Series of Monographs on Physics* published by the Oxford University Press.

While the standard of the printing of the text, figures and the large amounts of mathematics is first class, I have two criticisms of the presentation of the book. First, I believe that chapter and section numbers should be at the top of every page. Second, references take some time to find as they are given at the end of each chapter, often at least 100 pages or so further on. It is not easy to find the end of the chapter you are reading without labels at the top of the page.

I am sure the book will prove invaluable for all postgraduate students working in solid state physics and will be a constant source of reference for all other research workers. In common with most high-quality physics books today, it is priced well outside the range of an individual's pocket, but is an obvious definite buy for libraries and research groups with sufficient funds.

C. A. BATES

Physical Chemistry

A Textbook of Physical Chemistry. By Arthur W. Adamson. Pp. xxi+1079. (Academic Press: New York and London, 1973.) \$16.95.

THIS book covers, with a certain freshness of approach, much of the ground conventionally assigned to physical chemistry. Stress is appropriately laid on the need for precision in recording kinetic, thermodynamic and spectroscopic data. Each of the twenty-one chapters is followed by lists of references and numerical answers to problems. There is also a subject index to the whole book.

A slight danger lurks in the method presented for deriving certain integrated rate equations, but fears are allayed when it is once realized that, apart

from the type of print used, there is no real novelty. As in all books on physical chemistry, the question sooner or later must be answered: what level of mathematical equipment is it fair to take for granted? No clue is given, and a mathematical appendix to the text would greatly improve it. No use, incidentally, is made of the hyperbolic notation, in spite of its great convenience in dealing with partition functions. Inverse power functions frequently appear, though unnecessarily restricted to the integers $n=12$ and $m=6$, that is the Lennard-Jones potential. The brief summary of the activities and interests of Albert Einstein is hardly worthy of that genius. The subject is more fittingly dealt with in that celebrated mine of information published under the title of *The Born-Einstein Letters*.

Many of the anecdotal trivia, though well-intended, could be dispensed with, especially when they are as long as the particular article reproduced from the *Los Angeles Times* of 1955.

Among what can be described as major sins of omission is the complete absence of London's quantitative treatment of the heat of desorption of a molecule from a plane surface and from a hemispherical cavity.

All the diagrams have been eponymously redrawn. The copy sent to me for review purposes had no price on it and I am not beyond hoping that it may prove to be a priceless treasure to many readers.

The author, proof-readers, printers and publishers may feel justly proud of their work.

E. A. MOELWYN-HUGHES

Solid Batteries

Fast Ion Transport in Solids. Edited by W. van Gool. Pp. xvi+728. (North-Holland Publishing Company: Amsterdam and London; American Elsevier: New York, 1973.) Dfl.100.

THE key to many problems in the management of electrical energy lies in storage. For example, we would long ago have dispensed with the burning of petrol in cars if enough energy for long runs could be carried in batteries weighing about a hundredweight; the battery, again, is the most inconvenient element in the new electronic wrist-watch. For this reason, intensive research is proceeding on improving electrical batteries. One major advance would be to get rid of the liquid in batteries. If no vessel is needed, then everything can be miniaturized and new degrees of flexibility in design are possible. This book gives an account of a ten-day Summer School, on the main technological problem involved in producing a battery without liquids, namely the achievement of high

ion currents in solids. The contributors were about evenly divided between universities and industrial research laboratories, with a few from user agencies such as railways, defence, space and power-generating agencies. The book slightly condenses the twenty-three tutorial and thirty-seven research papers and adds accounts of discussions after each paper and a brief conference summary by R. A. Huggins. A very creditable effort has been made to capture the essence of an intensive discussion between diverse specialists on a subject of high technological interest and to publish it quickly. It is, of course, still a book for specialists, not undergraduates, for the tutorial papers do not add up to a grounding in the subject of batteries. Instead, mathematical treatments of ion transport in solids are balanced by, and often linked solidly with, experimental studies of transport in, or the structure of, materials exhibiting fast ionic transport (structure is important because the presence of a "crystallographic tunnel" is a prerequisite of rapid ionic transport). There are only a few theoretical treatments, such as papers by R. D. Armstrong and co-authors on the requirements for fast cationic transport, R. Kikuchi on theoretical techniques in ion transport, M. Rice on a theory of superionic conduction, and B. C. H. Steele and A. D. le Claire on diffusion theory. The selection of the papers has, in general, been well planned to cover the important aspects of fast ionic transport.

An important element in determining diffusion of atoms through solids is defect structure. Several experts discuss defects superficially and the concept underlies many of the papers on transport. A deeper treatment would thus have been desirable. The same can be said for intercalation compounds, which are promising electrode materials. It is unfortunate that there are virtually no general discussions of these compounds, although there is one particular attempt to use CrO_2 intercalated in graphite as a "sink" for sodium ions. Another system, classed under "promising new materials", is a polyaniline complex with a resistivity lower than 1 ohm-cm. Perhaps the dream of an organic semiconductor, with all the advantages of cheapness, low-temperature operation and fine adjustment by side-chain modification, may soon come true, but this book gives no guidance as to the prospect.

Perhaps the most stimulating paper is that by L. Heyne, who exhibits the rare ability to use sophisticated solid-state physical concepts and data (reflectance spectra, band-gap energies, defect levels, electronic conductivity, etc.) to aid in the selection of a "good" solid electrolyte. Likewise, Milberg's paper on ion

motion in glasses opened up some interesting vistas, and the article by J. W. Geus on "Transport Properties of Thin Films" was impressive in the clarity of its exposition but disappointing in that it stopped short of explaining transport through films as thoroughly as it explained surface physics.

This book is a good expression of current research thinking in ionic transport in a small range of materials and achieves a reasonable if not perfect balance between the present and the future of this field.

A. G. HOLMES-SIEDLE

Aerosols and Atmosphere

Aerosols and Atmospheric Chemistry. Edited by G. M. Hidy. (The Kendall Award Symposium Honouring Professor Milton Kerker at the Proceedings of the American Chemical Society, Los Angeles, California, March 1971.) Pp. xviii+348. (Academic Press: New York and London, November 1972.) \$14.50.

THE study of aerosols is now a science in itself. Each year one sees an increasing number of papers concerned with the formation, properties, behaviour and effects of aerosols. This follows from an increased awareness of the impact of aerosols on the environment and their involvement in varying fields such as meteorology, physiology and engineering. This book is essentially a collection of thirty-one papers presenting a wide variety of studies on the physical chemistry of aerosols and their relationship with atmospheric chemistry.

The book is in three parts. The first contains twelve papers on what might be described as general aerosol science. These papers cover the production of aerosols by X rays, the formation and properties of ultrafine particles and small ions conditioned by gaseous impurities in air, and also ultrafine oxide particle preparation in flame processes. Other topics covered are nucleation theories and studies of the kinetic growth of aerosol particles. Light scattering and the measurement of aerosol particle size distributions by other methods such as the spiral centrifuge are also discussed. This part ends with an interesting paper on the description of a new technique for the study of individual aerosol particles of diameters ranging from 100 to 500 nm held stationary in a laser beam by means of time varying electrostatic fields.

The second part of the book contains six papers which are mainly concerned with the application of basic aerosol science to atmospheric phenomena. Topics covered include the formation, growth and chemical reactions of atmospheric particles, the comparison of synthetic and natural smog aerosols, and

light scattering of photochemically produced aerosols.

The remaining thirteen papers in the third part of the book form a detailed discussion of the Pasadena Smog Study—a major investigation of Los Angeles smog carried out at the California Institute of Technology in 1969. Here there are papers describing the project, the data collection and aerosol analysing systems used, as well as the presentation of experimental data on aerosol size distributions and chemical compositions.

Factors such as climatology, meteorology, behaviour of trace gases and the mechanisms for particle-gas interaction are discussed in relation to their effect on the evolution of the Pasadena smog.

The book is clearly written and well presented, and has a useful subject index. It also lists the sixty-five names and addresses of the authors, and of these, fifty-one originate from the USA, nine from France, three from Germany and two from Canada.

I would expect to find this book widely read and used by all research workers in the field of aerosol science, and those interested in the subject of atmospheric chemistry with particular reference to air pollution.

It should be mentioned, however, that the book is an adaptation and reprint of the *Journal of Colloid and Interface Science*, **39** (1972).

ALAN B. HEDLEY

Electrostatics

Electrostatics and Its Applications. Edited by A. D. Moore. Pp. xviii+181. (Interscience: New York and London, May 1973.) £12.50.

THIS book, written by eighteen contributors, is intended to be a unified and complete survey of modern electrostatics, but it is not clear at whom it is aimed. The quality of the writing varies considerably, and the language is often woolly and obscure. Perhaps it is meant as a eulogy on electrostatics: the editor claims that without it the USA might have been part of the British Empire!

SI units are used mainly but not exclusively. The mixing of symbols for quantities and for units in equations can be confusing, like

$$V = \frac{Q}{C} V$$

There are no problems and only one worked example. On the credit side, most chapters begin with a helpful "overview". There are copious references, and most of the diagrams are clear.

The first four chapters give the basic concepts. These are presented in a confused fashion, and the lack of rigour hinders understanding. A few examples

from the many misleading and incorrect statements follow. It is said that if electrons are removed from atoms or molecules, positive ions or anions are formed, and anions move to the anode. The transfer of charge in a Van de Graaff generator is described, and then the statement is made that many examples of "convective charge transport" are found in other devices. The text in one case says that the velocity of accelerated particles depends on the square root of the charge, and then gives an equation showing (incorrectly) that it depends linearly on the charge. In the section on photoelectric charging it is said that "free electrons are not easily ejected from a surface because of the relative impossibility of satisfying both energy and momentum conservation conditions". Cross hatching is referred to in a figure which has no cross hatching. Mention is made of the "coupling between electrical forces and Newton's second law".

Chapter 5 is better. It gives a straightforward presentation of the theory of dielectrics, and considerable information on surface charges for a variety of materials. The practical problem of obtaining reproducible results is emphasized. Chapters 6 to 8 deal with long-lasting electrical effects, electrostatic motors, and electrostatic generators. Some interesting information is presented, but considerable space is given to historical descriptions at the expense of modern developments. The language is sometimes vague and even misleading. For a book published in 1973, it is astonishing that no mention is made of the contribution made by Ray Herb to electrostatic generators.

The remainder of the book deals mainly with the industrial applications of electrostatics, and the standard is generally businesslike. Chapter 9 deals with electrostatic precipitation, presents the relevant theory and gives a good deal of practical information. Chapter 10, on the electrostatic separation of solids, is mainly descriptive and at a fairly elementary level. The methods and equipment used in electrostatic coating, for painting, sandpaper manufacture, and so on, are described in chapter 11, and chapter 12 does the same job for electrostatic imaging. Chapter 13 covers the principles and elementary theory of impact printing, and discusses various practical systems.

Two of the most interesting chapters to me, perhaps because of my total lack of familiarity with the subject, deal with non-uniform field effects. A rather fundamental approach is adopted to this topic of dielectrophoresis, and then its application to biological materials (for example the separation of living yeast cells from dead ones) is discussed. Rather disappointing chapters on electrostatics in

the power industry and atmospheric electrostatics are followed by electrostatic nuisances and hazards, and a chapter on miscellaneous effects and applications, from electrostatically operated display signs to the effects of ions and electric fields on living organisms. The final chapter, by the eminent Professor Felici of Grenoble, reviews the blessings brought to us by electrostatics. He concludes his chapter and the book with the magnificent sentence, "Then applied electrostatics will reveal itself as a gentle, protective, and benevolent science, best suited to reconcile man with his environment and to help him along through the trials he is sure to face in the future".

It is an excellent idea to cover the theory and applications of electrostatics in one volume. This makes the disappointment all the greater that the treatment is so uneven.

R. G. P. VOSS

Nearshore Phenomena

Waves on Beaches and the Resulting Sediment Transport. Edited by R. E. Meyer. (Proceedings of an advanced Seminar conducted by the Mathematics Research Center, University of Wisconsin, and the Coastal Engineering Research Centre, US Army, at Madison, October 1971.) Pp. vi+462. (Academic: New York and London, October 1972.) \$16.

THE basic concept behind this volume is excellent: there is undoubtedly a need for a comprehensive review of the new ideas that have produced major advances in the understanding of nearshore phenomena in the last decade. This book therefore provides a welcome reference point for the modern work in this field. Unfortunately the compendium format has severe disadvantages when one is trying to present a balanced, interdisciplinary view of the state of any subject. Here, the fact that this is a collection of individual contributions is further emphasized by the lack of any obvious logic in the arrangement of the chapters. The general approach is inevitably uneven, some chapters concentrating on general reviews of a topic, some describing very specific pieces of research. The general level is therefore hard to assess; certainly the geologist or geographer interested in beaches will find the mathematical treatment rather formidable. The casual reader may have difficulty in appreciating the interrelation of the concepts presented in the separate chapters.

However, the more advanced student will certainly find this volume very useful. The chapters on wave recording, basic equations, wave caustics, run-up, wave breaking and longshore currents provide a general background for the

physical processes. The sections on wave resonance and harmonic generation are rather specific and, both being strongly mathematical, will probably be of interest only to the specialist.

The sedimentary chapters are far less satisfactory. Hayes's pictorial essay forms a valid introduction and should have been followed by a presentation of the current ideas on the processes which produce the sedimentary features. However, the chapters by Einstein and Kennedy and Locher both describe rather specific laboratory experiments and make little attempt to relate their measurements to the conditions on real beaches. It remains for Longuet-Higgins in the chapter on longshore currents to make the only interesting comments on sedimentary processes on beaches *per se*.

A. J. BOWEN

Organic Reactions

Selected Molecular Rearrangements. By T. S. Stevens and W. E. Watts. Pp. x+212. (Van Nostrand Reinhold: London and New York, 1973.)

MOLECULAR rearrangements comprise a diverse range of organic reactions and a one-volume review has not previously been attempted. The present work is not intended to be comprehensive, yet it covers a large fraction of the subject and includes some esoteric reactions. An introduction is followed by chapters on rearrangements of carbonium ions; nucleophilic rearrangements from carbon to nitrogen and oxygen; 1,2-electrophilic rearrangements and other alkali-induced processes; pericyclic reactions; and aromatic rearrangements.

Such a range necessitates often superficial coverage, but even so the standard is curiously uneven. The chapters on 1,2-electrophilic and related rearrangements which form the heart of the book are outstanding, and that on pericyclic reactions is good. However, the introduction could well be dispensed with and the second chapter has important glosses or omissions. Hydride shifts, the occurrence of "hot" ions in deamination and the controversy concerning classical and nonclassical ions are mentioned cursorily and often the discussion of other topics is uncritical: thus the Tiffereau modification is stated to be superior to the conventional Demjanov procedure, but no explanation is given. In addition, the discussion of aromatic rearrangements is inadequate and propagates the π -complex mechanism for intramolecular processes with no indication that this view is no longer accepted by the majority of workers in the field.

The book might also benefit by the introduction of the concept and terminology of orbital symmetry at the beginning (instead of on page 148); by the inclusion of more quantitative data

(Hammett parameters, migratory aptitudes); and by a distinction between the [2,3] and [1,2] sigmatropic forms of the Wittig and related rearrangements. There are also a few odd statements (how many would consider the Hofmann rearrangement to be "one of . . . the most widely-used reactions in organic chemistry"?), and the reading lists have some strange omissions such as Shine's definitive text on *Aromatic Rearrangements* and chapters from Thyagarajan's series on *Mechanisms of Molecular Rearrangements*. Indeed the review articles listed at the end of each chapter seem to have been rather arbitrarily chosen.

These criticisms aside, the book contains few errors and is very well written. However, the market is not obvious: too much detail is included for most undergraduate courses whereas research workers in any of the discussed fields will soon have to turn to the references for further reading. It is a pity that the authors could not have negotiated for an extra 150 to 200 pages. They could then have widened the discussion of the present material and included topics such as tautomerism, ionotropy, radical rearrangements and a few more specific reactions, for example, the Tiemann, Jacobson and sulphanilic-acid rearrangements, such as to give an essentially complete and valuable survey. Perhaps we can hope for a second edition along these lines?

D. V. BANTHORPE

Quantum Optics

Coherence and Quantum Optics. Edited by L. Mandel and E. Wolf. (Proceedings of the Third Rochester Conference on Coherence and Quantum Optics held at the University of Rochester, June 21–23, 1972.) Pp. xiv+911. (Plenum Press: New York and London, 1973.) \$43.

THIS volume consists of the papers given at the Third Rochester Conference on Coherence and Quantum Optics. It records an important gathering together of practically all of the leading figures in this most lively no-man's-land between traditional optics and quantum mechanics.

Yet was the book worth producing in this form? The large majority of the physicists now interested in the field were at the conference and obtained their information at first hand. For future researchers and those unable to attend, all the worthwhile material will surely be published in the usual journals. Some first class review papers (notably that of Jaynes on Neoclassical radiation theory) are made available, but the material is put together in such a haphazard fashion that they do not stand out. The volume is merely a compilation of preprints grouped by conference session with no proper introduction and

apparently no overall editorial hand. In some cases, potentially most interesting contributions (like Willis Lamb's "Critical Comments on Radiation Theory") are only present as abstracts. Furthermore, there is no record of the discussions following the papers, a necessity if the book is to stand as any sort of record of the conference proceedings.

Surely this type of publication is a scientific white elephant? Useful perhaps for impressing a participant's parent institution or for making an imposing presence on the bookshelf, it merely adds to the unnecessary lumber of publications which threatens to drown physics in a sea of paper. Because they are produced, these compilations are bought; but if they did not exist would the physics community be much worse off? D. G. C. JONES

Gravitational Waves

Gravitational Waves in Einstein's Theory. By V. D. Zakharov. Translated from Russian by R. N. Sen. Pp. xx+183. (Israel Program for Scientific Translations: Jerusalem; Halsted Press, a Division of Wiley: New York and London, 1973). £5.85.

THIS book, which the publishers describe as a "generalised critical review", is the first monograph exclusively concerned with gravitational waves. It consists of a chapter on experiments which has perhaps inevitably been rather superseded by present developments, a chapter on approximation methods, and a review of work on the rigorous mathematical theory of gravitational waves in general relativity. In particular much attention is directed to the problem of developing criteria by which a given space-time may be said to describe a gravitational wave. Nine such criteria are discussed together with the relationships between them and to the Petrov classification. This problem is closely connected with the Cauchy problem and the types of discontinuities that the Riemann tensor may possess. Also included is an account of Bel's super energy tensor. This section is well done, although there are a number of minor errors and misprints—the Penrose diagram is drawn incorrectly, for example.

Then follows a discussion of gravitational wave optics and the propagation of gravitational waves. Here one could have hoped for a clearer discussion of the Goldberg-Sachs theorem and the many exact solutions which have been discovered using it—for example, the important Kerr and Taub-NUT solutions. Indeed the statement on page 81 that the Robinson Trautmann solutions comprise all vacuum algebraically special gravitational fields is definitely

incorrect. There follows a section on plane waves according to various definitions and a chapter on the asymptotic properties of gravitational fields, Bondi mass loss, "peeling" and so on. Here there are several omissions—Penrose's conformal approach to the peeling theorems, the conserved (?) quantities of Newman and Penrose, definitions of mass and momentum and its relation to the BMS group which should really be included. Work on spinors is alluded to but not described and some Newman–Penrose quantities are incorrectly defined. The last purely theoretical chapter is devoted to the work of Zelmanov.

In his concluding remarks the author states that both from the point of view of experiment and theory much has to be done. With this one can only concur but one cannot help feeling that he is being a little pessimistic in his view of the "problem" of gravitational waves and a little misguided in his search for a unique criterion for gravitation waves—for example, a unique Petrov type. To demonstrate the existence of any kind of radiation it is surely only necessary to show that the physical field in question can transmit energy-momentum and information with a certain speed and without the necessity of a receiver (that is, not inductively). It was the achievement of Bondi and his group first to demonstrate this theoretically in a rigorous fashion; it is to be hoped that the experimental verifications will not be too long coming.

In conclusion then this is an admirable review, with a splendid bibliography, of some aspects of gravitational radiation theory. It is not for the astrophysicists seeking to find ideas for astronomical application or the practical man building apparatus but it should be well suited to people entering the field of mathematically rigorous gravitational radiation theory provided they supplement it with other reading. It would also help them to know that western relativists call "isotropic" "null".

G. W. GIBBONS

Environmental Poisons

Environmental Quality and Safety. Edited by F. Coulston and F. Korte. Volume 1. *Global Aspects of Chemistry, Toxicology and Technology, as Applied to the Environment.* Pp. x+267. (Georg Thieme: Stuttgart; Academic: New York, July 1972.) \$18.

CONCERN over environmental quality and the safety of chemicals released into our environment has been increasing steadily over the past few years. This is also reflected in the phenomenal growth in the literature on environmental pollution and other aspects. Any reasonable attempt at throwing some light on to the confused

scene of conflicting opinions presently existing in this field should, therefore, be given full support. Volume 1 of the new series on environmental quality and safety is a brave start if the declared intentions of the editors come to fruition. Perhaps by chance, most of the articles in the first volume are devoted to the toxicology of pesticides. Only one paper by Hasserodt concerns itself with the pollution problem created by man's needs for an ever increasing supply of energy to satisfy the demands of industry and to provide for his comforts. Two out of a total of twenty articles relate to pharmaceutical chemicals. They do not touch on the environmental aspects but are more concerned with standard safety assessments of drugs and profitability considerations.

Pesticides, their environmental behaviour and the implications of their use in agriculture as well as of their residues in food for man as the ultimate consumer, and for the environment generally, are covered in a competent manner. The articles by Matsumura, Brooks and Klein present clear and reasonably comprehensive reviews of the present knowledge of the metabolism of pesticides in microorganisms, insects and plants. However, some statements are not in accordance with presently accepted opinion. Thus on page 60, the assertion by the author, that 50% of the lead ingested in food is absorbed, is patently at variance with the generally

accepted figure of 10%. Furthermore, on page 61, to say that the major source of lead in our environment is derived from lead alkyls used in gasoline is incorrect. By far the largest amounts derive from natural geological sources and the enormous industrial usage of lead and its compounds.

How to handle the detailed information on pesticides and arrive at a sensible and satisfactory evaluation of the balance between the benefits and risks of pesticide usage is clearly explained in an excellent article by van Tiel. The ultimate hazard to man from a pesticide depends on his actual exposure to it and all possible metabolic transformation products, if these persist in human food as consumed. The important point is made that only if accurate analytical data are available, as well as adequate determinations of the range of dietary intakes for foodstuffs liable to contain pesticides residues, can the toxicological information be used to make valid and sensible assessments of the health hazards involved.

A reasonable beginning has been made towards more purposeful discussions of environmental hazards and their assessment, but judgment on the real value of this series has to be postponed until further volumes have appeared. Otherwise the book is well produced, the index is not extensive but is adequate and there are only a few printers' errors to mar the generally satisfactory impression. P. S. ELIAS

Solar Flare



Photograph of active region of the Sun taken in H α light. Material is shown being ejected close to the limb, photographed on May 22, 1972. From *The Quiet Sun*, by Edward G. Gibson (National Aeronautics and Space Administration: Washington, 1973, \$6.20).

Molecular Structure

Molecular Structure and Properties. Edited by G. Allen. (MTP International Review of Science.) Pp. 264. (Butterworth: London; University Park: Baltimore, 1972.) £10; \$24.50.

THE inevitable growth of the subject and literature has made the part played by reviews in the physical sciences particularly vital. When the field, as here, underlies a large proportion of chemistry and physics, the editor, as Professor Allen points out, faces severe problems in the selection of topics, which must indeed be to some extent subjective. A compromise is made here by the combination of surveys of well established work of continued importance with those of fields which represent growth points. "Neutron Diffraction" (G. E. Bacon), "Electron Diffraction by Gases" (K. Kuchitsu), and "Structure of Crystalline Polymers" (H. Tadokoro) represent long-established areas, while more novel fields are "Acoustic Studies of Molecular Conformational Changes" (A. M. North and R. A. Pethrick), "Magnetic Susceptibilities of Diamagnetic Molecules" (R. Ditchfield), and "Techniques and Uses of Infrared Refractivity" (J. Haigh and L. E. Sutton); related to the last, but curiously separated from it in the text, is a review of the theory of dielectric polarization (A. D. Buckingham).

Tadokoro considers with some systematic formality the results of the study of crystalline polymers, by X-ray and other techniques, during the past five years, in the context of the important problems which remain. Bacon, making no attempt at comprehensiveness in reviewing neutron diffraction, gives an authoritative forward-looking survey with illustrations from typical recent studies, and including molecular spectroscopy by inelastic neutron scattering. Kuchitsu's contribution is notable in that, throughout, it considers the collective evidence of electron diffraction and spectroscopic methods. Although a spectroscopist might question the literal truth of the statement in the preface that electron diffraction remains the major technique for all but the simplest molecules, there is no doubt that the most meaningful treatment of the structures of free molecules involves simultaneous application of diffraction and spectroscopic data. Those seeking the "best" values of geometric parameters in key structures are likely to refer to Kuchitsu here for some time.

Electron diffraction and spectroscopy have provided much information about the geometry and energetics of large amplitude motions within molecules, notably internal rotation or inversion of groups, and changes in ring conformations. The important and largely

complementary contributions made by acoustic studies to our knowledge of such motions, particularly when higher-energy conformations are weakly populated, are well brought out by North and Pethrick. The reviews of infrared refractivity and theory of dielectric polarization fittingly emphasise the bridge which must be made between the properties of free molecules and the bulk properties of materials.

The review by Ditchfield of magnetic susceptibilities of diamagnetic molecules is particularly welcome in bringing together aspects of many fields. As befits the current interest in the directional magnetic properties of free molecules, consideration of molecular orbital interpretations is included at some length, while the determination of magnetic anisotropies of molecules from crystal data, Cotton-Mouton studies and, particularly, the Zeeman effect in microwave spectroscopy is discussed along with the evidence of magnetic shielding in n.m.r. Anyone wishing to appreciate the growing contribution of detailed knowledge of molecular magnetism to our critical appraisal of descriptions of structure can start here.

All these chapters, which have been painstakingly produced, will be valuable to structure chemists, and to many who might not describe themselves in precisely that way. When one remembers that volumes 3 and 4 of the same series will concentrate on spectroscopy, one recognizes in the MTP Reviews an unusually well-balanced authoritative publication.

J. SHERIDAN

Pauli's Physics

Pauli Lectures on Physics. Volume 1. *Electrodynamics.* Pp. x+160. Volume 2. *Optics and the Theory of Electrons.* Pp. 159. Volume 3. *Thermodynamics and the Kinetic Theory of Gases.* Pp. x+138. Volume 4. *Statistical Mechanics.* Pp. x+121. Volume 5. *Wave Mechanics.* Pp. xiv+205. By Wolfgang Pauli. Edited by Charles P. Enz. Translated by H. R. Lewis and S. Margulies. (MIT: Cambridge, Massachusetts and London, March 1973.) \$9.95 each volume.

WOLFGANG PAULI was a great theoretical physicist who made many major contributions to the formulation of the key concepts of present-day theoretical physics. It is therefore of considerable interest to see his approach to the presentation of these concepts through the various lecture courses he gave to students at the Swiss Federal Institute of Technology in Zürich.

The five books reviewed here are excellent translations by S. Margulies and H. R. Lewis of lecture notes taken by different students and collaborators of Pauli. The notes have been carefully edited by C. P. Enz who provides

a preface and an appendix to each volume. The former sets out the circumstances in which the notes were made and the latter consists of a series of short technical comments on different parts of the text. A foreword to this series of books by Victor Weisskopf sets out the rationale for this publication of notes of lectures which, it must be recognized, were in general given more than twenty years ago. Briefly, Weisskopf says that "Pauli's way of presenting physics is never out of date", his style "is commensurate to the greatness of its subject in its clarity and impact" and his lectures "show how physical ideas can be presented clearly and in good mathematical form, without being hidden in formalistic expertise". There can be no argument with these views and the notes in these five volumes are remarkably concise presentations of what might be called basic theoretical physics and, as Weisskopf assures us, "they bear the mark of the master in their conceptual structure and their mathematical rigidity".

The level of each course goes rather beyond that of a standard undergraduate physics course bearing the same name in a British university but is probably at about the level found in theoretical physics options. The question naturally arises whether these volumes might be useful as "course" books. Here it is doubtful whether they would be for standard courses. They make no concessions to the reader, explanations are not elaborated and in any case it is assumed implicitly that the reader has been through a significant introductory course beforehand. In general the lectures embark at once on the setting up of a firm mathematical formalism and this style is continued throughout. As his students say in a preface to Volume 5, "Professor Pauli was fond of expressing himself with formulas and only a few words. Once he said 'One should not write so much'".

That being said, there is no doubt that the books could provide excellent "additional reading" and would be particularly valuable for the well-motivated and enthusiastic student. Equally they could provide a good basis for optional courses in theoretical physics either for physics or mathematics based undergraduates.

Together they are a remarkable testimony to the elegance of Pauli's approach to physics, his powers of exposition and his penetrating insight. I should certainly wish to associate myself with the concluding sentence of Weisskopf's foreword—"May these volumes serve as an example of how the concepts of theoretical physics were conceived and taught by one of the great men who created them".

R. J. BLIN-STOYLE

Tropical Atmosphere

The General Circulation of the Tropical Atmosphere and Interactions with Extratropical Latitudes. By Reginald E. Newell, John W. Kidson, Dayton G. Vincent, and George J. Boer. Vol. 1. Pp. xiii+258. (MIT: Cambridge, Massachusetts, and London, 1972.) £11.25; \$25.

A WORLD-WIDE sample of free-air balloon observations from 330 stations within about 45 degrees of the equator (1957–1964) has been analysed. The results are presented mainly in the form of climatological maps and vertical cross-sections, with some commentary. Volume 1 of this observational study contains chapters on the sources, processing, and analysis of the data; mean temperature and wind fields; the angular momentum budget and the maintenance of the zonal winds; and the seasonal variation of humidity parameters.

The text is well written and easily digestible, but the discussion is inevitably well removed from most of the maps and more frequent and specific references could have been incorporated. The cartographic work is of a very high standard.

The role of the tropics in the planetary atmospheric circulation, and the relationship between the zonal mean flow and the eddies, have always been two of the outstanding problems to those investigating the general circulation of the atmosphere. Theoretical and numerical studies must be based on our state of knowledge of the real atmosphere and this monograph adds a great deal to our knowledge and understanding of the mean circulation and eddy fluxes in the tropical and sub-tropical atmosphere in relation to the distribution of energy sources and sinks.

Although much-needed background values are established, the regional values are in some cases suspect. To a large extent this reflects the low density distribution of stations, particularly in the southern hemisphere and over the oceans; but the analyses could also have been improved by more critical linkage with other regional analyses and synoptic meteorology. For example, the only station in the sector 10° S–20° N, between 40–70° E, is Aden. This is precisely the sector in which the East African low-level jetstream is to be found, which transports nearly half of the total estimated transequatorial flow of air in the lower troposphere in July. The analyses of the $\bar{u}$ and $\bar{v}$ wind components at the 850-mbar level in summer show highly disturbing discrepancies with the more detailed analyses by Findlater. No jet core is shown along the coasts of Kenya and Somalia, whereas a jetstream with a mean southerly component of up to 15 ms⁻¹ has been shown to exist. Off the Horn of Africa the westerly component is

shown to be some 7 ms⁻¹ lower than in Findlater's analysis. To find a northerly component right across the Arabian Sea is equally as alarming. One is left wondering as to how near reality are the values in other regions, particularly over the oceans.

D. WINSTANLEY

Geology of Kilimanjaro

The Geology of Kilimanjaro. By C. Downie and P. Wilkinson. Pp. 253+10 plates. (Department of Geology, University of Sheffield: Sheffield, 1972.) £6.

KILIMANJARO mountain lying astride latitude 3° S in NE Tanzania attains a height of 19,340 feet, which makes it the highest peak in Africa as well as one of the largest composite volcanoes in the world. These facts attracted the attention of the local Geological Survey in the early 1950s, together with sporadic reports concerning fumarolic activity with sulphur deposits, a permanent but rapidly diminishing ice cap and the influence of the mountain on water supplies.

Following the Survey's preliminary work the problems were studied in greater detail during an extensive geological mapping programme in two field seasons in 1953 and 1957 by members of the University of Sheffield and the Geological Survey of Tanzania. From this field work a series of published and unpublished progress reports was produced and petrological studies and other research were carried out in laboratories. The results are now concisely presented and those responsible for the ultimate publication of this finally completed volume are to be congratulated. The work will undoubtedly become of great importance to many scientists besides East African geologists.

The chapter on glaciology, which is one of the five longer chapters, is lucidly written in fascinating detail by Downie and Humphries of Sheffield who incorporate K/Ar age determinations on volcanic rocks to aid dating of five important glaciations. In chapter three, Downie carefully reconstructs the regional geomorphology prior to the initial eruption. Additional borehole data will no doubt improve his picture of the "Kilimanjaro Depression", situated, more convincingly now, some 100 miles east of the great Gregory Rift Valley.

Excellent geological descriptions by Downie of the two highest peaks, Kibo (19,340 feet) and Mawenzi (16,896 feet), comprise two of the larger chapters, 6 and 7. Plutonic plugs, radial dykes and concentric sheets are significant features most carefully observed. Lahars originating from the three major volcanic centres, and on Kibo, a 10,000

foot complete geological sequence, are, in addition, magnificently displayed and recorded.

In chapter 9, Wilkinson indicates that no abnormal geothermal gradient is deemed necessary to assist in the considerable diminution of the Ice Dome. This contention will satisfy some, but lack of extensive accurate data on fumarolic activity is somewhat disappointing and perhaps underlines the difficulties of these problems anyway.

The last and longest chapter covering petrological studies is different because the actual concepts developed from laboratory data are now considered. Some fifty new, valuable rock analyses, including trace elements, are presented. Nevertheless, Wilkinson is undecided on some of the problems raised; for example, are up to four separate parent magmas really developed from a primary magma?

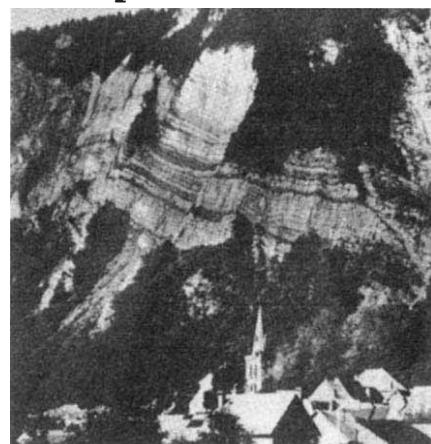
The shorter chapters, likewise instructive and full of merit, fit neatly into place, and cover, besides introductions, the geology of Shira and Ol Molog, the parasitic and subsidiary cones and a valuable final summary of the history of Kilimanjaro.

A comprehensive list of references and index of localities is included at the end of the volume. Opportunities for further research in many fields continually suggest themselves to the reader, for although old problems are now better understood new problems have appeared.

The text, with few significant errors, is very readable. Some splendid photographs are included—unfortunately none in colour—along with abundant, useful illustrations. A set of photomicrographs and a coloured geological map at 1/125,000, with Kibo details at 1/50,000, make the complete volume exceedingly good value for £6.

N. J. GUEST

Alpine Sediments



Thick Lower Jurassic sediments above Bourg d'Oisans in the French Alps; from *The Nature of the Stratigraphical Record*, by Derek V. Ager (Macmillan: London, September 1973, £2.95).

Physical Dogma

On the Explanation of Matter Wave Interference: Towards the End of Indeterministic Physics. By C. W. Rietdijk. Pp. xii+98. (Van Gorcum; Assen, The Netherlands, 1973.) 26 Dutch guilders.

THIS book does not really serve a useful purpose. One may agree with the author that in many respects science is far less rational and far more reactionary than one would wish it to be. As Planck put it in his autobiography: "New ideas are finally accepted, not because their opponents are convinced of the correctness of the ideas, but because they die out." One can also agree with the author that the Copenhagen interpretation of quantum mechanics sometimes greatly resembles a strong vested interest which by some of its adherents is defended with religious fervour. There have, however, been serious discussions of alternatives by eminent physicists, such as De Broglie, Bohm, and Belinfante, and a book which carries the subtitle "Towards the End of Indeterministic Physics" should at least discuss such attempts. Moreover, the discussion should take place in scientific journals. As the author quotes what he feels are objectionable statements by the establishment without giving references and as his arguments lack precision (for instance, retardation effects due to the finiteness of the velocity of light are neglected in a relativistic discussion), I find it very difficult to judge whether or not the author has made any significant contribution to this important discussion.

D. TER HAAR

Jumping the Gap

Electrical Breakdown in Gases. Edited by J. A. Rees. Pp. ix+291. (Macmillan: London and Basingstoke, 1973.) £7.95.

THIS book is comprised of a selection of twenty-five original papers (or extracts) surveying the subject of the electrical breakdown in gases. The papers are conveniently divided into two sections. The first thirteen papers deal with the pre-breakdown conditions prevailing in an initially uniformly electrically stressed gap. The remainder deals, much less thoroughly, with a miscellany of topics including the later stages of breakdown, breakdown in non-uniform fields, corona discharges, lightning and laser induced breakdown.

Most researchers today feel inundated by the written word and I can feel only sympathy for the editor who attempts to sift out just twenty-five papers for special mention. The value of such a collection, however, is crucially dependent on this very subjective selection. In my opinion the

first half of the book contains a well chosen array of papers. The classical contributions of such authors as Townsend, Raether and Meek are beyond dispute and these are complemented by an interesting selection of illustrative material to form a well balanced whole. The second part of the book is more controversial. The editor has chosen to include a wide range of topics and, in so doing, has made it difficult to develop them in a convincing manner. Difficult perhaps, but not impossible; the four papers on lightning are, I think, most instructive.

Though the selection of the contributions must be the major concern of editor and reader alike, it is vitally important in such a volume to mould the various papers into some form of contiguous whole. Dr Rees attempts to do this by inserting brief introductions to each paper. They are very brief and, in the first half of the book where the papers themselves have some continuity, I think adequate. But I found them insufficient to rectify the disjointed nature of some parts of the second half of this volume. To supplement the material of this book, the editor has appended short lists of references for further reading. These occur frequently throughout the volume and are of considerable value.

The success of such a collection depends on the purpose it sets out to fulfil. The editor feels that it should be of value to undergraduate and postgraduate students. There are several competing textbooks that seek to serve this same purpose and, in the task of learning the fundamentals of the subject, they are, I have no doubt, to be preferred. However, that is not to deny the interest that this book may engender. The student (as well as his mentor) usually meets the ideas expressed in this book in a mature form, well polished by constant repetition: certainly it is stimulating to recall the original in its more basic state. In a world in which time is not easy to find, however, I suspect that this book will be read as a pleasurable leisure-time activity more than as a utilitarian introduction to the subject.

R. M. BULL

Probability

An objective Theory of Probability. By D. A. Gillies. Pp. x+250. (Methuen: London, May 1973.) £3.50.

THE author clearly expresses the aim of this book at the beginning of his preface: "To present a philosophical theory of probability which can best be considered as a development of von Mises' views. I am entirely in agreement with von Mises in regarding probability theory as a mathematical science similar

to mechanics or electrodynamics, and probability itself as an objective measurable concept similar to mass and charge. In this sense I am presenting an objective theory of probability—as opposed to subjective or logical accounts."

The "subjective or logical" approach also of course has its adherents, for example, Harold Jeffreys or Bruno de Finetti; and the author discusses the rival traditions in the introductory chapter. He then, in part 1, develops Mach's operational approach to scientific concepts, spending perhaps rather excessive time, in a book on probability, on physical concepts such as force and mass. Part 2 contains the systematic development of "objective probability" as an analogous concept, with an axiomatic superstructure designed with the relation with observed frequencies in mind. While modern developments in the study of random sequences, for example, by Kolmogorov and Martin-Löf, are referred to, the author seems to adopt a point of view commonly held, associating the probability concept with a hypothesis of randomness. Whether probabilities can be usefully assigned to single events (Karl Popper's propensity theory, or Eddington's "localization of probability") is considered a somewhat secondary issue.

In part 3 the author faces the problem which this approach, as distinct from the alternative tradition, inevitably raises (it is not automatically to be condemned for that, or other scientific concepts would be similarly condemned): if the so-called objective probability is in part a hypothetical concept, how do we justify making use of it? He appeals to the "hypothetical-deductive approach" represented by Karl Popper and E. B. Braithwaite, in which scientific hypotheses are set up to be falsified rather than established. The special difficulty, that statements involving probabilities can never be strictly falsified, is discussed at some length (chapters 8, 9, and 10), with some final comments on the status of the Neyman-Pearson theory of testing hypotheses, as the author sees it. In spite of some sympathy with the author's general standpoint, I found this part of the book somewhat inconclusive reading. The book, as a whole, however, is very readable; even if it is unlikely to resolve so longstanding a controversy as to how probability should be used in science, or to justify the claim (see wording on the paper jacket) that "the book presents a new and rival theory of probability", the reader should have more understanding of probability after he has read the book than he did before. Past experience with other works on probability suggests that one should not expect more than this.

M. S. BARTLETT

BIOLOGICAL SCIENCES

Feeding the World

Farming and Food Supply: The Interdependence of the Countryside and Town. By Sir Joseph Hutchinson. Pp. x+146. (Cambridge University: London, June 1972.) £3.

World Agriculture in Disarray. By D. Gale Johnson. Pp. 304. (Macmillan: London and Basingstoke; St Martin's: New York, March 1973.) (Published in association with the Trade Policy Research Centre, London.) £3.50 cloth; £1.25 paper.

THESE are two important but sharply contrasting books. At a time when doubts are being voiced about the productive potential of world agriculture and when international prices of agricultural products have swung sharply upwards to unprecedented levels, it is necessary to keep in perspective the issues affecting world food supplies, as well as the relationship between the development process in less developed countries and international trade in agricultural products. Both of these books are especially relevant although the central message of each differs sharply from the other.

In his book, Sir Joseph Hutchinson refines and develops a series of lectures given by him when he was Drapers Professor of Agriculture at Cambridge. He has drawn on his long experience as an agricultural scientist and plant breeder particularly in Africa, south of the Sahara, and later in Britain and India, to discuss the technical basis, as well as the social and economic significance, of agricultural systems in those countries with which he is closely familiar. He starts by tracing the history of agricultural processes, analyses the physical and biological constraints on farming, and looks at farming systems in Britain, Africa and India. In all of this there will be much to learn for most readers, especially those not familiar with the processes of farming.

In the final section of the book, a strategy for agriculture and for feeding the people is suggested. This section represents more of a personal philosophy concerning the promotion of a more vigorous and effective rural economy with agriculture playing the central role in it. It is the section where some readers, with the experience of high income countries in mind, may find it difficult to accept the conclusions.

Using the British pattern of development as a model, the author shows how agricultural productivity increased as a result both of the adoption of new technologies and the shedding of labour to other industries as they developed. At the same time, as agriculture became more productive, it became possible to

feed an increased population. Such a process is seen as inevitable in the poor and less developed countries, although industrialization in the Western sense is not necessarily envisaged. Rather, a new balance is advocated for rural communities between farming and crafts, local industries and services. Moreover, a pessimistic view is taken about the contribution to development from any enlargement of world trade. He writes, "Supply and demand for food is a local matter." Others might say that this situation is coming about anyway as a result of highly restrictive agricultural policies in the richer industrial countries of the world so that important sources of income are denied to poor primary-producing countries, thereby impeding their economic development.

This is one of the main themes of the volume by Professor Gale Johnson who is a Professor of Economics at the University of Chicago and one of the foremost of the agricultural economists in the USA. In this book he examines such topics as the nature of agricultural change, agricultural policies in industrial countries and their effects on trade, and food production and needs in developed and developing countries. Throughout the book he develops strong and well-supported arguments against the nature of contemporary policies for agriculture in the developed countries, given the nature of the demand for food in richer countries and the high cost of much of the production encouraged by such policies in terms of resource use as well as in cost of products to the consumer or taxpayer.

Essentially the book is concerned with the conflict between domestic agriculture policies and trade policies. Given the importance most governments attribute to the enlargement of international trade as a means of achieving economic expansion, in both industrialized and less developed countries, Professor Johnson shows how such an objective is impeded. The pursuit of autarchic agricultural policies encourages farm production to the point of recurring surplus. This production causes considerable disruption to trade and leads to barriers being raised to restrict trade in agricultural products. Such moves are generally against the national self-interest of the countries concerned as well as the international intention to encourage development in the poorer countries. Speaking of the failure of many farm policies the author concludes: "The efforts to achieve appropriate objectives by inappropriate means results in massive interference with international trade and a significant diminution of the con-

tribution that agriculture could make to the welfare of people everywhere." The author goes on to suggest that much international trade negotiation today deals only with the symptoms of these policies—tariffs, import quotas and other restrictions to trade. Instead, such negotiations should deal with the underlying causes of these various restrictive devices and establish guidelines to help to liberalize trade in farm products.

Whether such an objective can be attained at a time when trade relations are as disturbed as at present between the major industrial groups of nations, as well as with the Third World, is for conjecture. What is beyond argument is that Professor Johnson makes a challenging case against prevailing agricultural policies of industrial countries. Unless steps are eventually taken to ameliorate the situation, then Sir Joseph Hutchinson's pessimism about the contribution to development to be expected from expansion of international trade is likely to prove well founded.

JOHN ASHTON

Portrait of a Protozoan

Blepharisma: The Biology of a Light-sensitive Protozoan. By Arthur C. Giese. Pp. x+366. (Stanford University Press: Stanford, California; Oxford University Press: London, July 1973.) £8.75.

EVER since the cracking of the genetic code, molecular biologists have been looking for fresh worlds to conquer. There has been a trend away from the former excessive concentration on bacteria and viruses. One group of organisms which have attracted interest as research material are the protozoa, the study of which, needless to say, has always had a long and respectable history in certain departments of zoology.

Restricting ourselves to one group—the ciliates—the most extensively studied genera are *Paramecium* (especially for genetics, though mainly of a rather unorthodox, non-Mendelian type), *Tetrahymena* (especially for nutritional biochemistry), *Stentor* (for morphogenesis), and now *Blepharisma*, the subject of this book.

Blepharisma is a very large cell, ranging from 50 μm to 450 μm in different species (some cannibalistic forms attaining 750 μm), swims slowly around in the water, and has very intricate surface structures, as revealed by the elaborate patterns of ciliary basal bodies or kinetosomes. It has striking powers of regeneration following injury or excision of parts, and is therefore very amenable to microsurgical manipula-

New Books from Blackwell

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Algal Physiology and Biochemistry

Botanical Monographs, Volume 10, edited by W. D. P. Stewart PH.D. D.SC. December 1973. 960 pages, 75 illustrations. £15.50

Biology in Pest and Disease Control

Thirteenth Symposium of the British Ecological Society, edited by D. Price Jones M.SC. PH.D. F.I.BIOL. and M. E. Solomon M.SC. F.I.BIOL. Early 1974. 272 pages, 23 illustrations. £6.50

Quaternary Plant Ecology

Fourteenth Symposium of the British Ecological Society, edited by H. J. B. Birks M.A. PH.D. and R. G. West M.A. PH.D. F.R.S. October 1973. 256 pages (oblong), 144 illustrations. £13.00

Two new journals

Journal of Immunogenetics

Published bimonthly at £15.00 (\$50.00) per annum post free. Volume 1, Number 1. January 1974.

Journal of Biogeography

Published quarterly at £10.00 (\$30.00) per annum post free. Volume 1, Number 1, March 1974

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tion. These advantages have been thoroughly exploited by Dr Giese and his colleagues, and a great deal of knowledge about differentiation has been obtained. Put very roughly, control of differentiation comes from two sources: (1) the macronucleus, and (2) various regions of the surface itself. It is very curious to read how the behaviour of the macronucleus is affected by surface manipulations, through the operation of some unknown feed-back mechanism.

Attempts are made to provide a theoretical basis for these processes, in current molecular biological terms, but these are at the moment somewhat speculative, and do not seem to have led to any fundamentally new ideas, either in molecular biology or in developmental biology.

The core of the book is the part dealing with morphogenesis, but there are many other interesting sections—such as those dealing with the ability of *Blepharisma* to regulate its physiological state in response to changes in the external medium; cannibalism; encystment; and to the consequences of a pigment "blepharismisin", rendering the organism sensitive to light, and making possible study of photodynamic phenomena. Notably absent is any information on genetics, because, according to the author, of the fact that nobody has yet succeeded in producing conjugation at will.

The book also provides chapters on morphology, ecology, taxonomy and other background topics. Least satisfactory is the section dealing with the alleged self-duplicating properties of kinetosomes.

Any biologist wishing to turn his attention to an organism having great potential value for new discoveries should have a look at this book.

G. H. BEALE

Melanocytes

Pigment Cell. Volume 1. Mechanisms in Pigmentation. Edited by V. J. McGovern and P. Russell (Series Editor: Vernon Riley). Pp. xiv+414. (Karger: Basel, München, Paris, London, New York and Sydney, 1973.) £15.45.

THIS volume contains the papers presented at the Eighth International Pigment Cell Conference, held in Sydney in March 1972. The first conference was held in 1946; and these meetings were organized by Myron Gordon until his untimely death in 1959. It is remarkable that such an International Pigment Cell Group should have survived so long in the absence of formal organization. The papers read at previous conferences have been published by a variety of publishers, and in different formats; but it has now been agreed that future proceedings should

be published in a uniform series, of which the book under review constitutes volume 1.

It may be asked, what makes pigment cells so interesting? The most conspicuous product of melanocytes is melanin, which acts as a protective screen against ultraviolet radiation, although its colour may not be so attractive as the pigment of green plants. Although apparently very stable chemically, it may well be that melanin possesses other functions, which are not yet understood. Moreover pigment cells are also useful as models for studying universal cellular activity.

Present day knowledge of the functioning of pigment cells has been attained through attack on the problem by a range of workers representing a wide spectrum of scientific disciplines, going all the way from organic chemists to clinicians, the latter including pathologists, dermatologists, and surgeons. Between these extremes lie studies in biochemistry, enzymology, physiology, pharmacology, cytology, genetics, and embryology; and considerable use has been made of electron microscopy. For this reason it is appropriate that papers related to the pigment cell should be published together in one place. Indeed a newcomer to the field would find it difficult to obtain an introduction into it except through this (and earlier volumes). The book presents the latest views of many authorities; and by following up the references given, earlier reviews and original papers will be easily located.

The present volume carries the subtitle "Mechanisms in Pigmentation", and about half of the fifty-one papers are related to morphological and physiological aspects of pigmentation. A considerable amount is now known about the control of the degree of melanin pigmentation in the skin, in which melanocytes and keratinocytes form a cooperative system, designated as the epidermal melanin unit. The melanocyte's contribution begins with the synthesis of melanosomes, and the oxidation of tyrosine to melanin. In the past the latter has been considered to be catalysed by tyrosinase, although one paper presents evidence to suggest that peroxidase may be involved in some cases. Melanogenesis is under hormonal control. Later the pigment granules are transferred from melanocytes to keratinocytes. A number of papers discuss the inter-relationship between melanocytes and keratinocytes in skin and hair, the chemistry of melanin, and the metabolism of tyrosine, while others deal with the role of cyclic AMP and various inhibitors and activators of melanogenesis.

The remaining half of the papers are concerned with human melanoma, including immunological aspects, the

biochemical manifestation of melanoma in blood and urine, experimental pharmacology, and the epidemiology of melanoma, including variation in ethnic and geographical incidences.

The book is excellently produced and can be warmly recommended to all those wishing to obtain an understanding of the pigment cell.

G. A. SWAN

Electromyography

New Developments in Myography, Electromyography and Clinical Neurophysiology. Edited by John E. Desmedt. Vols 1-3. Pp. xxvii+2094. (Karger: Basel, London, New York and München, 1973.) Sw. francs 600; £76.20; \$186; DM 560.

THIS work is a collection of papers originally presented at the Fourth International Congress of Electromyography in Brussels in 1971, supplemented by others written subsequently to make the coverage of the subject more complete. In a monumental work of editing, Professor Desmedt has brought together 168 papers and has achieved this quickly enough to ensure that the material is still quite fresh.

Volume 1 starts with several papers on new concepts of the motor unit. Much of this is concerned with the correlation of histochemical fibre type with physiological properties. Real advances in this field are evident now that microanatomical and physiological studies are being done together in the same laboratories. The glycogen exhaustion technique of Kugelberg and Edstrom has been decisive here in showing that the fibres of a unit are widely dispersed but all of the same type. The concept of subunits revealed by features of the normal electromyograph is now abandoned, though the idea finds new application in the abnormal type grouping occurring after partial denervation. As this new understanding of the motor unit develops, caution is needed in the rigid classification of unit types. The objective basis for specifying them is not yet well defined in quantitative terms and one wonders how general the scheme evolved by Burke of three distinct types will prove to be. The difficult problem of order of recruitment of different units in voluntary contractions has been tackled by Warmolts and Engel by open muscle biopsy combined with electromyography. It appears that the large, fast, ATPase-rich units can only be activated by brief strong efforts, though it is questionable whether this means that such units are not accessible to really strong maintained contraction.

As there is no general discussion of papers recorded from the congress it is a pity that the editor has not provided some comment on apparently conflict-

ing accounts. Many readers would have appreciated some enlightenment on the confused fibre type nomenclature. Did the apparent assumption by McComas and his colleagues of unit size and type homogeneity in their human muscle studies go unchallenged? Did Engel and Warmolts, who have otherwise made such valuable contributions, escape unscathed after their presentation of their ideas on diseases affecting the motor unit "*in portio or in toto*"?

Two papers by Stalberg and Ekstedt are of outstanding interest in giving data obtained with the single muscle fibre technique. It is now possible to understand the microphysiology of the normal unit much better and, in disease, to decide whether fibres in a unit are inactive due to defects in the fibre itself, the neuromuscular junction or in the terminal nerve branches. This technique is sure to become more widely applied and is already giving new information on muscle conduction velocity and on the F response (volume 3).

In recent years, a number of methods, which at first sight seem only suited to animal experiments, have been applied to human material. The advances which can result are well shown in the article by Elmqvist on transmission defects studied in human intercostal muscle biopsy specimens. The application of such results is obvious in the succeeding long and scholarly paper by Desmedt on myasthenia. The first volume continues with some not very illuminating articles on kinesiology but ends on a happier note with useful exploration of ways of analysis of electromyograph data and changes in fatigue.

Volume 2 starts with a collection of papers on nerve conduction, studied both with the now conventional techniques and using the highly refined averaging methods of Rosenfalk and Buchtal and the application of cerebral evoked potentials by Desmedt and his colleagues. The micro-neurographic technique makes a contribution in two papers on single unit studies of human skin receptors, while for single motor fibres, Bergman describes his ingenious finely graded stimulation method. There follows a series of clinically orientated papers on the application of conduction velocity studies, showing how useful even such an apparently simple objective measurement can become in clinical neurology, when it is properly made and understood. I feel that the papers on EMG studies of sphincters seem to make the converse point.

The final section devoted to automatic EMG analysis with computers is disappointingly small. There are a few papers from the other volumes which could have been included here and some other contributions might have been

solicited to make the section more representative of what seems likely to become an important field.

Volume 3 contains a succession of papers by workers of great prestige in the field of muscle control. There is an ordered sequence from neuroanatomy of the motor system through studies of animal and human muscle spindles to recent work on pyramidal and extra-pyramidal systems, and a valuable review of the servo theory, relating to respiratory muscles. The first four chapters on tone, spasticity and rigidity show how great is the gap between modern neurophysiology and the clinical scene, but the clinician studying the subsequent articles must be impressed by the efforts being made to close it. This is nowhere better illustrated than in the article by Vallbo on normal human muscle spindle action followed by one on the application of the same elegant technique to studies of spindle activity in Parkinsonian patients. Much of the remainder of this volume is devoted to a series of papers on reflexes studied in man. These will be valuable to any clinical neurophysiologist interested in quantitative recording of reflexes, especially because of the generally careful discussion of method and interpretation of electrical responses.

In all, the three-volume work, ponderous and expensive though it is, represents a notable contribution and many neurophysiologists will be grateful to Professor Desmedt for his labours.

A. TAYLOR

Eight-legged Beasts

Pycnogonids. By P. E. King. Pp. 144. (Hutchinson: London, August 1973.) £3.50.

A MAJOR fascination in the study of invertebrate zoology resides in the small groups. Each represents an experiment in the disposition and functional interplay of the essential organ systems and each has been successful—up to a point. It may have had its period of success only to be superseded and yet to linger on like the brachiopods. Or, like the chaetognaths, it has been successful but in too narrow a sphere to permit further evolution.

The pycnogonids provide an admirable instance of another such group. One was taught about a section of the arthropods having eight legs—but certainly not arachnids—entirely marine and with bodies so narrow that the digestive organs penetrate the legs and where the males take over responsibility for guarding the developing eggs. On early marine courses one later encountered the little *Pycnogonum littorale* often addressed to the surface of sea anemones on which it fed, or else the excessively thin-bodied and spider-like species of *Nymphon*.

There personal contact was likely to end, leaving one unaware that there are over 500 species of pycnogonids contained in eight very distinct families. Their major environment is the deep ocean floor. Over the oozes which cover this they walk on stilt-like legs while direct development from large yolk eggs frees them from dependence on planktonic larvae.

Nor are the legs always confined to four pairs. As far back as 1835 the American zoologist Edw. S. Stimp described specimens with greater numbers but these were dismissed as curiosities until later South Polar expeditions dredged both decapodous and dodecapodous species within three distinct families.

In these days students hear little more than passing mention, if even that, of pycnogonids. It is well that the substance of what is known about them should be so ably described and clearly illustrated by P. E. King in this latest volume on marine biology from Hutchinson. It can be strongly recommended to all who work in the sea, indeed will bring enlightenment to all zoologists.

C. M. YONGE

Gonad Anatomy

Postnatal Development of the Ovary in Homo sapiens and Macaca mulatta and Induction of Ovulation in the Macaque. By Gertrude van Wagenen and Miriam C. Simpson. Pp. 306 (122 plates). (Yale University Press: New Haven, Connecticut and London, 1973.) £11.75.

THIS book by two eminent researchers in the field is a sequel to an earlier volume by the same authors entitled *Embryology of the Ovary and Testis: Homo sapiens and Macaca mulatta*. Like its predecessor, the present volume contains over four hundred photomicrographs of excellent quality which illustrate changes in the microanatomy of the ovary during the period from birth to senescence. The legends to the illustrations are largely self-explicit without referring to the text, although some appear to convey a different interpretation. Thus an inspection of the illustrations without reference to the text would give the unwary reader the impression that germ cells are formed throughout juvenile life from within epithelial cords derived from the somatic (so-called germinal) epithelium, when in fact the authors eventually come firmly to the conclusion that this is not the case (see below).

Only a small part of the sixty pages of text and tabular material is actually concerned with the subject given as the title on the cover of the book (four pages on human ovary; twelve for monkey). This section consists of a concise account of structural changes affecting the coelomic epithelium, follicles and stroma, as well as the differen-

tiation of germ cells. As such it forms a useful extension to the information conveyed in the first volume in the series. However, in some ways the text is disappointing; some of the references to figure numbers are incorrect and the bibliography is inadequate for a book of this type. It is particularly off-putting that the authors continually build the "Aunt Sally" that neoformation of germ cells from somatic components within the ovary continues long after birth, because they eventually present good evidence (and seem themselves to firmly accept the view) that this is not the case. This first section of the text also contains good accounts of changes in the morphology of the corpus luteum with the phases of the menstrual cycle, and on the occurrence of polyovular follicles.

The bulk of the text consists of an account of a whole series of experiments, conducted over many years, on the effects of various gonadotrophic hormones on the ovary of the rhesus monkey. These experiments were performed with the aim of inducing ovulation and the authors discuss the pitfalls of overstimulation resulting in superovulation. It is particularly interesting to those working in the field of oogenesis that the authors show conclusively that the number of primary follicles is not increased following treatment with FSH. It is to be regretted, however, that the amount of hormone injected is sometimes expressed in "rat units" instead of mg or international units, although this may well reflect the age of some of these studies, many of which have not been published before. Nevertheless, this section of the book will be of value to endocrinologists and reproductive physiologists in providing data for a species about which little information was previously available.

In summary, this book has a number of minor shortcomings but provides a considerable amount of useful information which is not readily available else-

where. As an atlas of the developmental microanatomy of the ovary it is unsurpassed and will be a valued possession of all those concerned with the development and endocrinology of the ovary.

T. G. BAKER

Enzyme Mechanisms

The Study of Enzyme Mechanisms. By Eugene Zeffren and Philip L. Hall. Pp. xi+284. (Wiley-Interscience: New York and London, June 1973.) £7.50.

THIS book attempts to cover a wide range of topics related to the study of the mechanism of enzymatic catalysis. As well as a series of accounts of enzymes for which plausible mechanisms are available there are sections on purification, chemical modification, kinetics, co-enzymes and allosterism. In order to cover such a wide range of topics in a relatively short book the authors have had to be selective in their choice of material. The section on enzyme purification omits any detailed discussion of a number of methods that are in common use and the chapter on the mechanisms of oxidoreductase is restricted to theories which involve proton transfer. In some cases the authors have oversimplified the situation. This is particularly evident in the chapters on kinetics, which only deal with enzymes that have a single substrate, and in the chapter on allosterism, which only considers the theory of Monod, Wyman and Changeux with the more general theory of Koshland mentioned only parenthetically as being "some variation of it". There are very few errors in the book but the chapter on the kinetics of enzyme inhibition contains some misleading information in which steady-state and equilibrium conditions are not sufficiently distinguished and in which mixed and partially noncompetitive inhibition are confused.

The strongest feature of the book is the series of chapters directly concerned with enzyme mechanisms. These com-

Mountain Plants



Alpine gentians: from left to right, *Gentiana Clusii*, *Gentiana alpina*, *Gentiana angustifolia* and *Gentiana Kochiana*; illustrated in the third volume of the *Flora der Schweiz* (*Plumbaginaceae bis Compositae*) edited by H. E. Hess and E. Landolt and illustrated by Rosemarie Hirzel (Birkhäuser Verlag, Basel and Stuttgart, 1973, Sfr 188).

prise a general chapter on the types of mechanisms which may be involved and a series of "case studies" of individual enzymes which occupy much of the latter half of the book. As well as the obligatory accounts of chymotrypsin and lysozyme the mechanisms of carboxypeptidase, some oxidoreductases and the fructose diphosphate aldolases are discussed. The experimental evidence and the proposed mechanisms are clearly and fairly discussed and the authors are careful to point out any inconsistencies between the mechanisms and the experimental observations.

Despite the lack of depth in many of the earlier chapters, the authors' clear and enthusiastic approach to the topics covered results in a stimulating work which should encourage the reader to delve more deeply with the brief but carefully selected references provided.

K. F. TIPTON

Hybrid Cells

Hybridization of Somatic Cells. By Boris Ephrussi. Pp. 175. (Princeton University Press: New Jersey, 1972.) £2.50.

THOSE who wish to read about cell hybridisation can now read books by Harris and by Ephrussi based on lectures given at Harvard and Princeton respectively. Ephrussi goes to some lengths to claim precedence for Barski over Watkins and Harris, a precedence of five years; but there is some difference in hybridity between the mouse-mouse hybrid of Paris and the man-mouse hybrid of Oxford; and, although this was more recent, it was certainly more hybrid.

Both books are well produced, and, if the space taken up by Ephrussi in claiming the priority of his work is excluded, are of similar length. Ephrussi's, which is two years later, has the advantage of being able to quote and evaluate more recent work, but there is little, apart from malignancy, which is not also covered by Harris.

Malignancy is interpreted in terms of a failure of established differentiation, and the differentiated cell is defined as one which has "luxury" activities as well as "housekeeping" activities, to use Ephrussi's polarising terminology. Hybrids between malignant and non-malignant cells were studied, but, using mice and pre-banding cytological techniques, it was difficult to interpret the nature of the inactivation of some malignant cells through hybridisation, and the decisive observation by which sins of omission could be distinguished from those of commission was not possible.

Those who have already read Harris's book may find the contrast interesting: those who have not should start with the earlier book. J. H. EDWARDS

Genetics of *Oenothera*

Oenothera: Cytogenetics and Evolution. By R. E. Cleland. Pp. x+370. (Academic: New York and London, December 1972.) £7.

ALTHOUGH Ralph Cleland began his research with significant studies of red algae, his life work was devoted to the evolutionary problems presented by *Oenothera biennis*. This was begun well before he visited Hugo de Vries in Lunteren in 1927-28. De Vries's work seems to have influenced Cleland much more at first than did that of Renner, whose seminal work was published in 1917. The cytogenetics of *Oenothera* has faded from the view of modern genetics, but it still has an immense importance in the theory of evolution and population structure. It is not impossible that it is on the verge of a significant revival. The permanently heterozygous systems developed in *Oenothera* have been regarded as blind endings in evolution. Nevertheless, recent work of Levin, Howland and Steiner has shown that *Oenothera biennis*, the ultimate in permanent heterozygotes maintained by complex interchanges, displays a surprisingly high degree of diversity in cryptic genetic variation as demonstrated by studies of its allozymes. The fitness for particular conditions possessed by current populations has been achieved, it would seem, at the possible expense of future wellbeing of the plants. The *Oenotheras* with extreme interchange heterozygosity have seemed to many of us to have run into an evolutionary blind alley. But perhaps there is an escape, for some of the genome, especially that at the ends of the chromosomes where chiasmata are concentrated, is capable of fairly free recombination. There seem to be potent possibilities for future evolution, other than the infrequent outcrosses between races which Cleland supposes. Evolution may not be slowed.

Oenothera was first important in the promotion of the mutation theory of evolution. Most of the mutants were not changes in genes but changes in chromosome number—polyploidy and aneuploidy principally. While genetic changes of this kind are sometimes important in evolution, they are not the material on which evolution commonly works. Dr Vries's work, described in the first two chapters, contributed much to assessing the place of mutation in evolution.

Cleland's third chapter, also in part I, "Genetical Behaviour of *Oenothera*", gives an account of Renner's work on *Oenothera* and his development of the theory of complexes. Like de Vries, Renner was at first, and perhaps predominantly, a physiologist. However, stimulated by Richard Goldschmidt's erroneous explanation of patroclosal inheritance in *Oenothera*, Renner was

led to undertake experiments of his own which showed that the various species were permanent heterozygotes, each generally producing two gametic complexes, each different from the other in a large number of genes. The cytological basis of these peculiarities, with the fourteen chromosomes forming rings at meiosis and alternate chromosomes passing together to the same spindle pole at division I, was first demonstrated by Cleland in 1922. The explanation of the cytology, based on Belling's concept of segmental interchange, was fully developed by Darlington in 1929 and 1931. The analysis of the relation between the arrangement of the pairing segments of the complexes in a wide range of species was begun by Emerson and Sturtevant in 1931 and developed with great enthusiasm by Cleland. This work is summarized in part II, and part IV, "Evolutionary Considerations", applies the results to population studies in the widest possible way. There is much here not otherwise published and of great interest to all concerned with the principles of evolution.

The third part of Cleland's book includes accounts of the supernumerary chromosomes found in some races, the genetic analysis of the Renner complexes, position effects, self incompatibility, plastid behaviour and the genetics of certain inconstant characters.

Dr Cleland's book is to be recommended as a source of detailed evolutionary information on the genus and as a summary account of the biological importance of the various historical studies which have contributed to our understanding of *Oenothera*. It should be read by all concerned with the study of evolution. D. G. CATCHESIDE

All About Interferon

Interferons and Interferon Inducers. Edited by N. B. Finter. Pp. xx+598. (North-Holland: Amsterdam and London; American Elsevier: New York, 1973.) Dfl. 80; \$25.

THIS volume contains information on everything you always wanted to know about interferon. The successor to *Interferons* published in 1966, this completely revised edition retains the core of contributors but very little indeed from the text of its successful predecessor. It has grown to almost twice the size of the previous edition, mainly as a result of the inclusion of new chapters and extensive subject and reference indexes.

As a possibly unintentional hint at its apostolic nature, the book was written by twelve contributors—all of them well known experts in the field. The relative uniformity in the general outline of individual chapters and the liberal use of cross-references reveal

frequent and well placed strokes of the editor's pen. The aim has undoubtedly been to publish an almost encyclopaedic authoritative reference work covering the developments in the entire interferon field up to the time of the writing of the individual contributions. This goal has largely been met. Omissions of significant work published before 1970 seem to have been few.

There is a wealth of detailed factual information. Thus, a total of sixty-six different viruses is listed under the heading "Some [*sic*] viruses which induce interferon", arranged in a table spread over four pages, and also including information about the animal species in which interferon production was reported, with the appropriate references. An individual wishing to accumulate this information on his or her own would probably have to spend weeks toiling in the library. A chapter entitled "Factors Influencing Interferon Production" (by M. Ho) occupies more than thirty pages.

Interferon research is a mixed bag. Rooted in virology, it has been branching out increasingly into other areas of microbiology, as well as chemistry, cell biology, molecular biology, immunology and tumour biology. These different facets of interferon work are represented in the book, although the main emphasis is on the role of interferon in the natural defence against viruses (chapter by S. Baron) and a considerable portion of the treatise is devoted to the description of antiviral effects of interferons and inducers in man and animals (N. B. Finter). There is, however, a new chapter on non-viral substances which induce interferon (T. C. Merigan). Also most welcome is the addition of a chapter by M. N. Oxman reviewing on some 90 pages interferon production by tumour viruses and tumour cells, as well as various aspects of the action of interferon and interferon inducers on tumour viruses and tumour cells, both in animals and cell cultures.

Most chapters were apparently written in about 1970. The delay in publication, the editor explains in the preface, was mainly because some contributions had been received late. Unfortunately, the hiatus has already made obsolete portions of the otherwise nicely written chapters on the mechanisms of interferon production (D. C. Burke), purification (K. H. Fantes) and mechanism of action (J. A. Sonnabend and R. M. Friedman). A further consequence of the delay is the absence of any description of some biological activities of interferon on uninfected cells which have only been recognised in the past three years.

The bibliography, listing 1,500-odd references, is arranged in alphabetical order and includes the complete titles of articles. Each reference is followed

by the page numbers on which it is cited in the book, so the bibliography also serves as a sort of an author index: a clever idea. The subject index is both extensive and well arranged.

The book will be appreciated mainly as a reference guide for those who have more than a passing affair with interferon. Portions of the book, however, can be useful to readers who wish to obtain more general information about some aspects of interferon work; they will benefit from the very readable introductory chapter outlining the chronology of major developments in interferon research (K. Cantell) and from the neat summaries at the end of each of the seventeen chapters. JAN VILCEK

Mycoplasma Monograph

The Biology of Mycoplasmas. By Paul F. Smith. Pp. x+257. (Academic: New York and London, August 1971.) \$14.50; £6.75.

In the preface, the author states that his aim is to acquaint the general biological scientist with a group of microorganisms which could be used to study basic biological mechanisms. Because of this, we have a monograph which deals more comprehensively with the biochemistry and physiology of mycoplasmas than any recent book or review on these microorganisms. Although Professor Smith disclaims any intention to provide a reference book, he has assembled and integrated the biochemical information so ably under appropriate headings that the monograph should prove useful for reference. In this connexion, the inclusion of further sub-headings in the contents would be helpful.

The author's interest in the origin of mycoplasmas is obvious in both the first and last chapters; he favours the highly plausible hypothesis that mycoplasmas have evolved from bacterial ancestors by losing the rigid cell wall and that this has enabled them to establish a nearly true parasitic relationship with their hosts. Comparisons of mycoplasmas with some existing bacteria have failed to show any relationship, but Professor Smith points out the limitations of the comparisons so far made. Although a relationship with some remote bacterial ancestor seems more likely than a relationship with an existing bacterium, the author's arguments may serve to counterbalance the present tendency to discount the possibility of any relationship between mycoplasmas and bacteria.

The inclusion of a chapter on the relationship of structure to function (chapter 4), an aspect largely ignored in recent books on mycoplasmas, is commendable. This correlates, as far as is possible at present, the information on structure and the dynamics of growth

and reproduction provided in chapters 2 and 3. Diagrams illustrating the author's theory of membrane permeation and two types of respiratory pathway are especially helpful.

The section dealing with host-parasite interactions in chapter 5 is less detailed than the rest of the monograph. This subject is of limited interest to cell biologists, however, and has been more fully covered in two recent books.

I should like to have seen the generic name *Acholeplasma* applied to those mycoplasmas which do not require sterols for growth, and the name *Mycoplasma* reserved for those which are sterol-dependent. This reluctance to adopt the new nomenclature may, however, be related to Professor Smith's theory on the relationship of sterols to polyterpene biosynthesis in mycoplasmas, a theory which is most compellingly argued in chapter 3.

Mycoplasmas are already being exploited by biologists interested in membrane function and structure. It is to be hoped that this book will direct the attention of other cell biologists to the potentialities of mycoplasmas in the study of biological mechanisms. In this connexion, the speculations in Professor Smith's final chapter on the significance of mycoplasmas as cells are stimulating and could provide a basis for further experimentation.

RUTH M. LEMCKE

Abyssal Animals

Abyssal Environment and Ecology of the World Oceans. By Robert J. Menzies, Robert Y. George and Gilbert T. Rowe. Pp. xxiii+488. (Wiley: New York and London, May 1973.) £12.50.

THE abyssal environment, although most representative of the Earth's surface in terms of area covered, is yet almost certainly the least known. A growing interest in the abyssal fauna reflects increasing concern of its possible perturbation, by exploitation of deep mineral resources and the dumping of industrial waste, when its origin, structure and metabolism are still virtually unknown to us.

This book traces the beginnings of deep-sea biology. It also includes notes and photographs of contemporary workers judged to have contributed significantly to the subject. Sampling devices are reviewed, but a balanced discussion of the most successful samplers in current use is not provided. The various schemes for classifying the bathymetric distribution of the marine benthos as a whole are outlined and an original hierarchical system, based on the rate of change of the component genera and species in terms of the percentage of taxa common to samples, is proposed. Discussion of faunal distribution in the northwestern Atlantic is based mainly

on the authors' intensive studies off the Carolinas. There is a short discussion of the northeastern Atlantic based on European work in the Bay of Biscay. The chapter titled the southeastern Pacific is based on work on a small number of faunal groupings in samples from a limited area off the coast of Peru. Discussion of the Arctic Ocean is based mainly on the shelf fauna because very few benthic samples have been taken from the deep Arctic basins. The authors reflect the modern view of the deep trenches as geologically unstable areas, which is supported by their low degree of faunal endemism above species level.

The book discusses abyssal biomass and the structure of the abyssal benthic community. Interesting, and previously unpublished, data from deep-sea photographs on spatial pattern of epibenthic species are included, as is some discussion on the measurement of faunal diversity and the interpretation of the results. The little and fragmentary information available on specialization in functional morphology, feeding, reproduction and vision that is associated with the deep-sea habitat is summarized.

The authors finally attempt to classify marine distributions into a scheme relating geographical range, depth zonation and temperature conditions, and then discuss the origin and evolution of the abyssal fauna. However, much of the material appearing earlier could have been more tidily incorporated into these sections. The book as a whole suffers from poor organization and an irritating style which makes it difficult to use for reference. It is a pity that the authors have not restrained their use of unfamiliar and often abbreviated jargon. Although the reader may eventually manage to make sense of sentences such as: "The UAZ always appears to have fewer species than the MAZ but more than the LAZ" (p. 251), he may well have succumbed to a mental constipation before reaching a later section titled "Eurybathial, Eurythermal Eurygraphs" owing to the clumsy style of writing. For example, the caption to Figure 1-22a does not make it clear whether the date of death given refers to the Russian gentleman or to the abyssal sipunculid worm that he is shown examining in the photograph. On page 92 the sentence: "The genera and species considered to be intertidal are those found along the shore between tide marks" would, by any reasoning, seem unnecessary.

Many of the deep-sea photographs reproduced resolve insufficient detail. Drawings depicting the larger animals in their habitat are given but it is doubtful whether they succeed in conveying more information than a good photograph.

There is a heavy emphasis on the Iso-

poda in this book. Although they are probably an important faunal grouping in the deep ocean, the authors might have produced a more useful and certainly briefer text as a monographic treatment of the marine isopods.

JOHN D. GAGE

Ancient Mosses

Bryophytes of the Pleistocene: The British Record and its Chronological and Ecological Implications. By J. H. Dickson. Pp. ix+256 (21 plates). (Cambridge University: London, February 1973.) £12.80; \$37.50.

MOST Quaternary botanical studies have been concentrated, naturally and properly enough, on the vascular plants of the period, and most workers in this field have thus been specialists in the identification of vascular plant remains. Mosses have sometimes been ignored, and some identifications have been amateurish, while others have been made by bryologists unused to naming fragments. In this book, by an authority in the field of Quaternary bryology, not only are the records assembled from a widely scattered literature, but many of the specimens on which they are based have been re-examined and many data are presented.

Introductory chapters covering the present distribution patterns of British bryophytes, geological and vegetational history, and materials and methods, are followed by a systematic survey, species by species, listing all subfossil occurrences and discussing their significance in relation to present-day British and continental distribution. The final chapters treat the subject chronologically, tracing the changes from pre-glacial times onwards. The book is well produced and lavishly illustrated—some reduction in the size and number of both distribution maps and plates could well have been made. The text is not easy reading, and for the non-specialist who does not carry in his head the precise definitions of some of the terms—Hoxnian, Neogene, Devensian, Anglian—a glossary would have been helpful.

Differential preservation makes interpretation of the record difficult. Only eighteen liverworts are known from the Pleistocene, against 179 mosses, and many of the mosses with no sub-fossil records are common ones. Some curious problems are raised. Why, for example, did the late Bronze Age men of the Humber select, of all species, *Neckera complanata* for caulking their boats? The information in this book has been well worth assembling, for the present distribution of British bryophytes cannot of course be understood without reference to their history, but it throws rather disappointingly little new light on the more general problems of the Pleistocene.

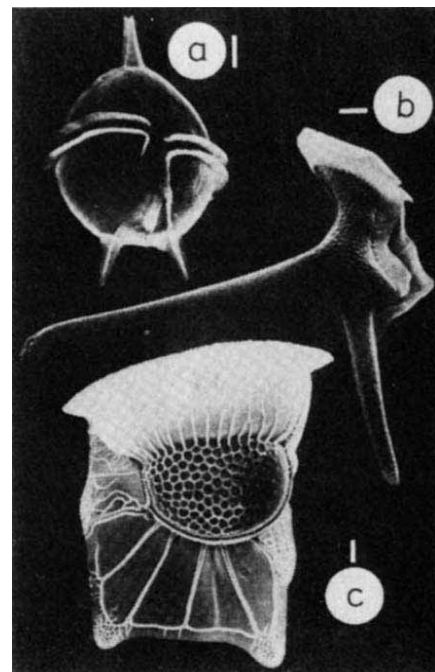
A. C. CRUNDWELL

Memory

Problems of the Psychology of Memory. By A. A. Smirnov. Scientific and Translation Editor: Samuel A. Corson. (Monographs in Psychology: an International Series.) Pp. xi+338. (Plenum: New York and London, 1973.) \$26.

Problems of the Psychology of Memory is a translation of a Russian work on the natural history of the memorization process in the tradition of Bartlett's classic work. The major part is a monograph, originally written in 1945, which has been slightly revised for the translation from the Russian. Its style is very heavy and the experiments described lack elegance. Despite this, if it had been published in English before the early 1960s, during the Dark Ages of the verbal learning approach to memory, it could have had a useful influence. Now, its basic position—the emphasis on the effect of the activity of the memorizer on memory—is standard in cognitive psychology. Much more incisive demonstrations of most of its claims have been produced by Western psychologists such as Miller, Tulving, Mandler and Paivio. The book stresses the importance of comprehension, of planning and of the nature of the orienting task on memory and there is some discussion of visualisation. It contains some protocols of the

Tropical Plankton



Scanning electron micrographs of dinoflagellates from the Indian Ocean. a, *Peridinium latispinum*; b, *Dinophysis miles* var. *schroeteri*; c, *Ornithocercus quadratus*. Taken from *The Biology of the Indian Ocean*, edited by Bernd Zeitzschel (Chapman and Hall: London; Springer-Verlag: Berlin, Heidelberg, and New York, August 1973, £16.35).

memory methods used by actors and an interesting anti-associationist view of mediators.

Two short essays complete the book. One on retroactive inhibition, written in 1940, is very dull. The other on the Russian work on the interrelation between verbal processing and imagery is more interesting and less antiquated but made somewhat dubious by the lack of any information-processing type concepts.

This book therefore cannot be recommended for any type of readership. On the evidence of this and two others in the same series it is unfortunate that Russian work on the higher mental processes has only been translated long after it could have had a beneficial effect on Western work. Whether translation of recent Russian work would be more productive remains unclear.

T. SHALLICE

Ancient Egyptians

Population Biology of the Ancient Egyptians. Edited by D. R. Brothwell and B. A. Chiarelli. Pp. 560. (Academic: London and New York, June 1973.) £7.

THIS volume contains thirty-two papers, twenty-nine of which, presented at a symposium held in Italy in 1969, have already appeared in the *Journal of Human Evolution*. Pagination is chaotic, and typographical errors abound. Some could create difficulties for students checking sources. Glaring, although relatively innocuous, is the ascription (in both the text and bibliography of Gray's paper on the radiography of mummies) of Harris's work on bone growth to H. A. Hains. The sequence of subjects follows no consistent plan. It is regrettable that a work commemorative of Sinton should not be better produced since it contains much of great interest.

The papers cover almost every aspect of the biology of the Ancient Egyptians and present them as a homogeneous group, living in relative isolation and a stable environment throughout a very long period. They thus afford a unique opportunity for the study of human micro-evolution in response to a limited number of identifiable factors. Inevitably the papers vary in value. Among the most stimulating are the demonstration by Raikes and Palmieri of the stability of the physical environment for at least 10,000 years; the indication by Masali and Chiarelli of differing mortality patterns in the Pre-dynastic and Dynastic periods; the controversial claim by Nemeskéri that population growth in Egypt and Eurasia depended solely on a fall in mortality rate (factors affecting fertility must play a part!) and the study by Saffirio of Egyptian diet and its calorie content.

Interesting papers based on work with skeletons and mummies include the studies by Bourke and by Satinoff of skeletal trauma and its treatment (the claim of the latter that the Egyptians could not reduce impacted fractures because of the lack of anaesthetics seems far-fetched, when even amputations were effected in the past without them); the re-emphasis by Borgognini-Tarli of the uncertainties of blood group determination on ancient remains; the salutary critique by Sandison of attempts to diagnose pathological conditions in surviving soft tissues; the excellent papers on skeletal and dental evolution and pathology (Greene's is a most meticulous study) indicating early occurrence of third molar suppression, an epidemic outbreak of caries concomitant with the domestication of cereals, and sexual convergence in bodily morphology following the expansion of agriculture; the contribution by Michelin-Lausarot to the new field of biochemical study of tissue breakdown in preserved organic material; the paper by Stouhal containing data possibly important for the study of the racial history of East Africa. Davide's list of mummies, skeletons and skulls throughout the world is valuable and its appearance here may lead to its amplification, since many small museums house specimens. The book is a very useful compendium.

H. HUGHES

Notes on Nutrition

Notes on the History of Nutrition Research. By Clive M. McCay. Edited by F. Verzár. Pp. 234. (Stuttgart: Berne; Hans Huber: Vienna, 1973.) 42 francs; 38 DM.

THIS book is needed for reference in libraries dealing with nutrition in the widest sense. It contains the notes for lectures on the history of nutrition studies by Professor Clive M. McCay of Cornell University, and has been published owing to the enterprise of Professor F. Verzár and the late Professor Maynard. I had the privilege of knowing McCay during a sabbatical period which he spent in my laboratory in Oxford, and on subsequent visits to him. He was an original thinker as well as being a fine scholar and a courageous man. Science has lost an important work through the illness which prevented him from finishing his book. As it is, the notes do not go much beyond 1920; they deal in order with fats, nitrogen, protein and inorganic substances. McCay has dealt in depth and with penetration with literature going back 150 years and earlier, so that it is an education to read through these notes. It is an effort which should be made by anyone who wishes to lecture with depth on nutritional subjects. Some of

McCay's most original work dealt with the effect of nutrition on longevity; rats were shown to remain much younger if fed less than normal amounts of a complete diet during the period of growth. I have found a few misprints, none of which can mislead the reader.

RUDOLPH A. PETERS

Foraminifera

Distribution and Ecology of Living Benthic Foraminiferids. By John W. Murray. Pp. xii+274+12 plates. (Heinemann: London, June 1973.) £7.50.

THE successful application of foraminifera to subsurface problems in the oil industry has led to a burgeoning interest in their recent ecology and to the second book in a decade on their world distribution. This compilation differs in being based entirely on records of living specimens. Data from different authors are brought together and presented in a uniform manner largely through diversity plots, based on the now unfashionable Fisher α index, and triangular plots of the main suborders. This is a valuable synthesis though the individual sections are rather brief and tend to fall between the requirements of the research worker and the interested non-specialist.

The restriction to living records raises the problem of adequate sample control. Methods are still on the whole crude and stations sparse. The Phleger corer which provides the necessary undisturbed surface material only works on certain bottoms. Furthermore, living individuals are generally swamped by the dead. This has driven the author to abandon the counts of 250–300 individuals recommended in the Appendix and settle for 100. Even then many samples fall short of the required numbers though comment on them is included. These difficulties would better have been fully discussed in the introduction.

Perhaps partly because of these problems, results are expressed in terms of relative per cent rather than absolute numbers, in spite of the admitted difficulties and the trouble taken to standardise area/volume. Unfortunately, this tends to perpetuate the unwarranted notion of a reciprocal relation between species. They may be neutral rather than competing.

The second paragraph of the preface should not be taken too seriously because palaeoecologic reconstruction is necessarily based on the fossilised remnant of the death assemblage. Indeed, the thanatocoenose may better represent total foraminiferal production over a period, precisely because of current mixing, despite the "veil of the Pleistocene".

JOHN R. HAYNES

Solar Radiation Changes and the Weather

J. W. KING

Radio and Space Research Station, Ditton Park, Slough SL3 9JX

It is suggested, on the basis of pressure measurements, that energetic solar particles which penetrate the atmosphere in the north Atlantic region may be responsible, in some unknown manner, for certain meteorological variations.

METEOROLOGISTS have conjectured for decades that secular variations and trends in the Earth's climate may be associated in some way with changes in solar radiation occurring during the eleven-year sunspot cycle; early work on this subject has been comprehensively reviewed by Craig and Willett¹.

It seems, however, on reading recent literature²⁻⁶ describing the results of major analyses of meteorological parameters, that the apparently very strong connection between the solar cycle and the weather is not generally appreciated. Here I present evidence which indicates that important climatic features, such as droughts and unusually long 'growing seasons', are dependent on the solar cycle to such an extent that significant progress could be made in forecasting the occurrence of these features if some account were taken of the expected levels of solar activity in the future. I also demonstrate that energetic particles may constitute one form of solar radiation which profoundly affects the lower atmosphere.

Solar Activity and the Weather

Figure 1 shows how the length of the annual 'growing season' (that is, the portion of the year during which the air temperature 1.25 m above ground exceeds 5.6° C) at Eskdalemuir (55° N, 3° W) varied during the years 1916 to 1969. This information was obtained from yearly values published by Gloyne² for 1914-71; these data have been smoothed by taking five-year running means and, therefore, smoothed results for the two years at the beginning and at the end of Gloyne's series are not available. Also shown in Fig. 1 are the yearly mean sunspot numbers for the same period. There can be little doubt that the length of the growing season (which is a useful parameter for evaluating the impact of climate on agricultural production²) varies during the solar cycle; the season is on average about 25 d longer near sunspot maximum than near sunspot minimum. A more detailed comparison of the two halves of Fig. 1 reveals the geophysically interesting fact that the growing season tends to be longest approximately a year after sunspot maximum. Gloyne pointed out that the growing season began very early in six years which he regarded as anomalous; these included 1938, 1948, 1957 and 1959 which were years of high solar activity.

Of the twenty-five sunspot maxima which have occurred since 1700, the 1957 maximum was the largest and the

1947 maximum the third largest. The 1969 maximum was relatively small and I therefore suggest that meteorological data acquired during the two sunspot cycles between 1944 and 1964 would be more suitable for investigating the relationships between solar activity and the weather than would data acquired since 1964; 1944, 1954 and 1964 were sunspot minimum years.

Figure 2 reveals additional information about the correlation between the solar cycle and the growing season at Eskdalemuir; the upper half shows the length of the growing season for the years 1944-64 compared with the day number on which each season began (that is, the day in each year on which the smoothed temperature first reached 5.6° C) while the lower half shows the growing season compared with the day number on which each season ended. The length of the season is almost entirely dependent on the behaviour in the spring and it may be inferred, therefore, that the solar cycle influences the temperatures in spring rather than in the autumn.

One of the most comprehensive analyses of meteorological data acquired during the two sunspot cycles between 1944 and 1964 would be more suitable for investigating the relationships between solar activity and the weather than would data acquired since 1964; 1944, 1954 and 1964 were sunspot minimum years.

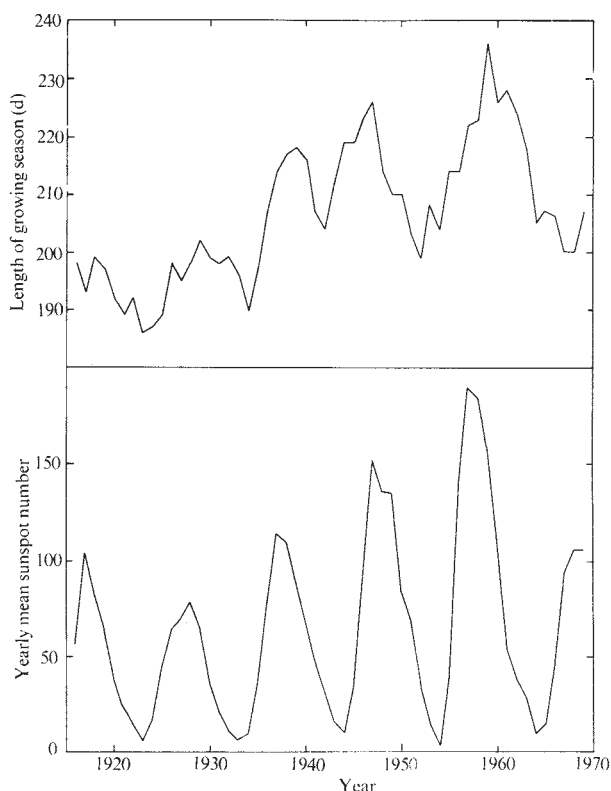


Fig. 1 Upper, the length of the "growing season" (that is, the number of days in the year on which the averaged temperature exceeds 5.6° C) at Eskdalemuir (55° N, 3° W); the points show the five-point running means of the yearly values reported by Gloyne². Lower, yearly mean sunspot numbers.

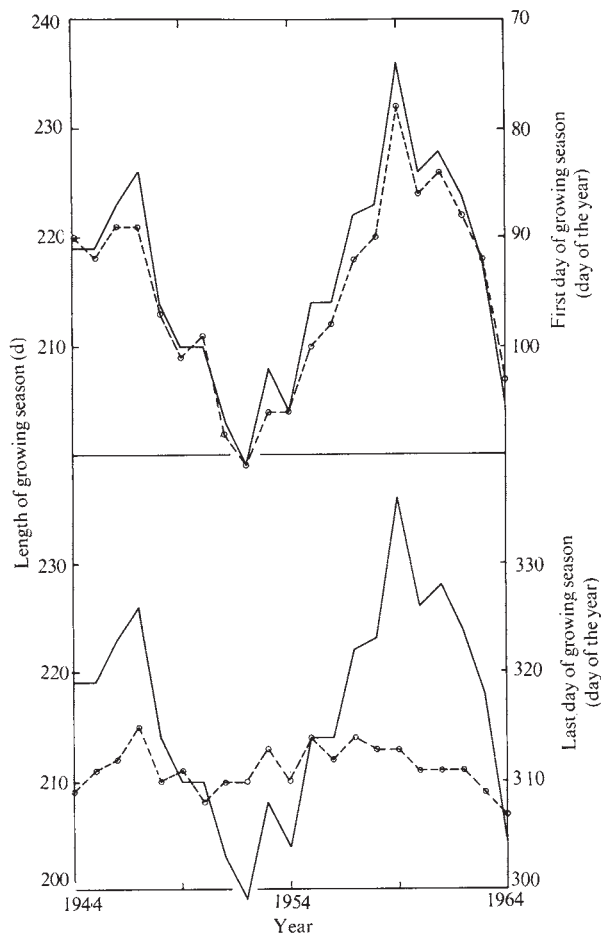


Fig. 2 Length of the growing season compared with the day number on which the averaged temperature first reached 5.6°C in each year (upper) and with the last day on which the temperature reached 5.6°C (lower). All data are five-point running means calculated from Gloyne's yearly values. —, Length of season; ○ --- ○, first day.

logical temperatures ever made is that reported by Starr and Oort³ who used a total of about 10 million individual measurements of temperature to obtain, for each of the 60 months between May 1958 and April 1963, the average temperature of the bulk (92%) of the atmospheric mass in the northern hemisphere. They subtracted the mean seasonal variation from the monthly values to obtain the 'residual' temperatures plotted in Fig. 3 and they drew attention to the decreasing trend which is such that the average temperature fell by 0.60°C over the five years. The cause of this trend, they pointed out, is a "field wide open for speculation". The monthly mean sunspot numbers for the 60 months have also been plotted in Fig. 3 and a comparison of the temperatures and solar data immediately suggests that the declining temperature trend may be caused by the decline in solar activity. One particular feature of both sets of data which suggests that this is so is the fact that in the first year and the last year of the five-year period the smoothed variations (lower half of Fig. 3) of both the temperature and the sunspot number are relatively flat. It is interesting, in view of the obviously high correlation between the smoothed temperatures and sunspot numbers, that there is only slight evidence that the two parameters are correlated on a month to month basis: some of the months in which the temperatures were relatively high or low are also months in which high or low sunspot numbers occurred.

Other meteorological data exist which indicate the existence of a general decline in temperature during the years

investigated by Starr and Oort. Bradley⁴, for example, has shown from his own observations and those of other workers^{7,8} that the average elevation of the freezing level (the 0°C isothermal surface) over the Canadian Arctic region fell dramatically between the period 1957–62 and the years 1963–66.

Another recent analysis of meteorological data, for the years 1951–1969, has been reported by Winstanley⁵ who concluded that during the winter–spring seasons (October–May) a high negative correlation existed between (a) the height of the 500 mbar surface along 40°N between 110°W and 70°E and (b) the rainfall over north Africa, the Middle East and north-west India. Figure 4 shows some of his data which fall within the interesting period 1944–64; also plotted (broken curves) are the smoothed monthly sunspot numbers corresponding to the mid-points of the October–May periods investigated. The altitude of the 500 mbar surface seems to be highly correlated with solar activity while the rainfall at Beirut is highly negatively correlated with solar activity. The rainfall was low during a period of several years centred approximately a year after sunspot maximum. It must be conceded that the data reported by Winstanley for the period after 1964 do not fit into the solar cycle pattern evident for the years 1951–64, but this may be because the 1969 sunspot maximum was very small compared with those of 1947 and 1957.

It is perhaps worth examining briefly the extent to which rainfall in England is dependent on the solar cycle. In order to do this, I have referred to Wales-Smith's⁶ analysis of monthly rainfall totals for the 274-yr period 1697–1970; this contains Tables showing the years in which the six wettest and six driest months, seasons and so on, occurred

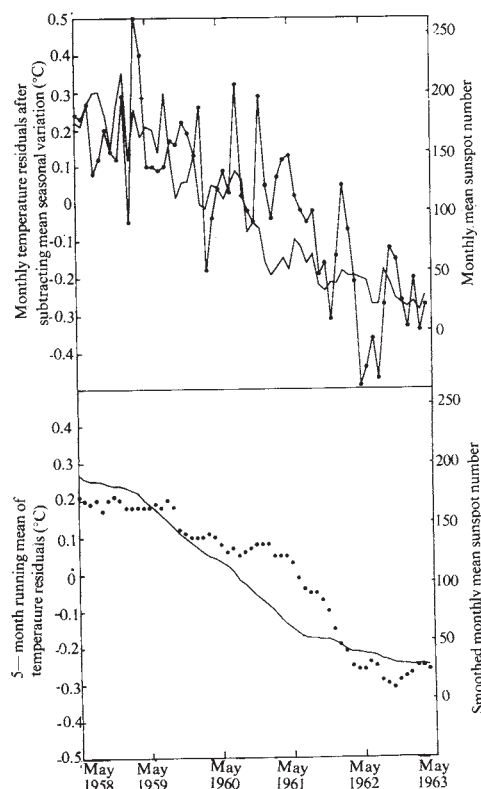


Fig. 3 Upper, monthly temperature residuals, calculated by Starr and Oort³, showing the declining trend in the average temperature of the lower atmosphere in the northern hemisphere. Also plotted are the monthly mean sunspot numbers. Lower, smoothed variations of the data shown above. The two points at the beginning and at the end of the temperature curve are necessarily not quite accurate. The smoothed sunspot numbers are the conventional 12-month running means. ●, Temperatures; —, sunspot numbers.

at Kew (51° N, 0° E). The solar activity corresponding to the years in which various abnormal rainfall totals occurred is shown in Fig 5. Figure 5a relates to the driest winters. These seem to be associated with low sunspot numbers; of the eighteen years, four were sunspot minimum years and seven others were the year immediately following a sunspot minimum year. Figure 5b shows, in contrast, that the driest springs, summers and autumns are associated with high solar activity; seven of the eighteen years were sunspot maximum years. (The highly statistical nature of this type of analysis becomes apparent when it is pointed out that, in spite of the evidence of Fig. 5b, the driest summers during the past 100 yr did not occur predominantly during active solar conditions.)

Figure 5c shows the sunspot numbers corresponding to the six driest winters, driest years, wettest winters and wettest years; these all tend to occur around sunspot minimum and, in fact, not a single one of the twenty-four dates occurred in the upper half of a sunspot cycle. At Kew, therefore, abnormal winter and annual rainfalls occur during quiet solar conditions. In view of the apparent dependence on solar activity of the date on which the growing season at Eskdalemuir begins, it is interesting to study Fig. 5d which shows that the wettest and the driest April or spring weather occurs mainly at times of very low or very high solar activity; of the twenty-four different years involved, seven were sunspot minimum or maximum years and twelve were years immediately adjacent to a sunspot minimum or maximum year. I conclude that abnormal rainfall in spring occurs at the extremes of the solar cycle, although the individual data show that the driest springs tend to occur

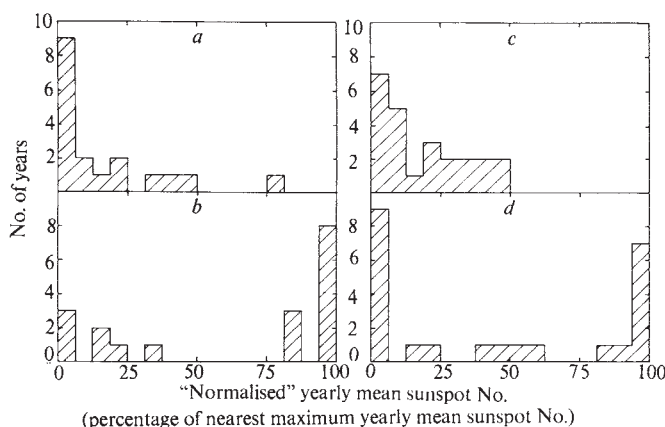


Fig. 5 Histograms of the yearly mean sunspot numbers corresponding to years in which abnormal rainfall conditions at Kew (51° N, 0° E) were reported by Wales-Smith⁶. (The numbers have been normalised by expressing them in the form $100(S-N)/(X-N)$ where S is the sunspot number, N the nearest minimum sunspot number and X the nearest maximum.) a, Six driest October–March periods, 1697–1970; six driest winters (Nov.–Feb.), 1697–1970; six driest winters (Nov.–Feb.), 1871–1970. b, Six driest springs (Mar.–Apr.), 1871–1970; six driest April–September periods, 1697–1970; six driest autumns (Sep.–Oct.), 1871–1970. c, Six driest October–March periods, 1697–1970; six wettest October–March periods, 1697–1970; six driest years, 1697–1970; six wettest years, 1697–1970. d, Six driest Aprils, 1697–1970; six wettest Aprils, 1697–1970; six driest springs (Mar.–Apr.), 1871–1970; six wettest springs (Mar.–Apr.), 1871–1970.

near sunspot maximum while the wettest springs tend to occur around sunspot minimum.

Further evidence of the fact that exceptional weather occurs at the extremes of the sunspot cycle has been provided by Baur⁹, who showed that sunspot minimum and sunspot maximum years are the times of most probable occurrence of severe winters in the north-east United States and in north-central Europe. Some evidence exists that the English summer may behave in a corresponding manner. Information contained in Wisden¹⁰ can be used to show that, of the twenty-eight occasions on which cricketers have scored 3,000 runs in a season in England, sixteen have been in sunspot maximum or minimum years; the five years in which this rare phenomenon occurred more than once were all sunspot minimum or maximum years. Likewise, thirteen of the fifteen occasions in which a batsman has scored 13 or more centuries in a season took place in, or within a year of, a sunspot maximum or minimum year.

Energetic Solar Particles and the Weather

It is now well established^{11–14} that the density and temperature of the atmosphere above about 100 km vary markedly with solar activity during the 27-d solar rotation periods and eleven-year solar cycles. Jacchia¹¹ has shown that the average value of the exospheric temperature increases from about 750 K at sunspot minimum to about 1,200 K at sunspot maximum; the 27-d oscillations have an amplitude of approximately 200 K. Even though such effects in the upper atmosphere are well known, the physical mechanisms by which corresponding effects might be produced at lower heights are not clear.

Meteorological evidence exists, however, which can be used to demonstrate that solar radiation in the form of energetic particles may influence the weather. The direct entry of solar wind particles to the near-environment of the Earth varies with the magnetic invariant latitude (not to be confused with magnetic latitude, geomagnetic latitude or magnetic dip) and has been observed^{15,16} to be most pronounced at invariant latitudes of 75° to 79° near local

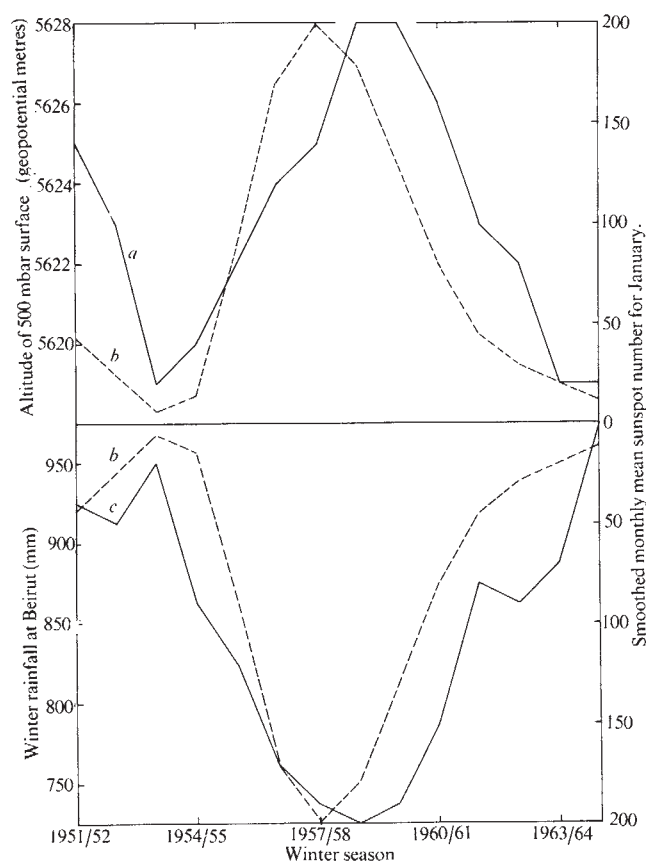


Fig. 4 Plots showing, for October–May periods, the altitude of the 500 mbar surface at 40° N between 110° W and 70° E (upper section) and (lower section) the rainfall at Beirut (34° N, 36° E) (after Winstanley³). Also plotted are the 12-month running mean sunspot numbers centred on January. a, Altitude of 500 mbar surface; b, sunspot number; c, rainfall.

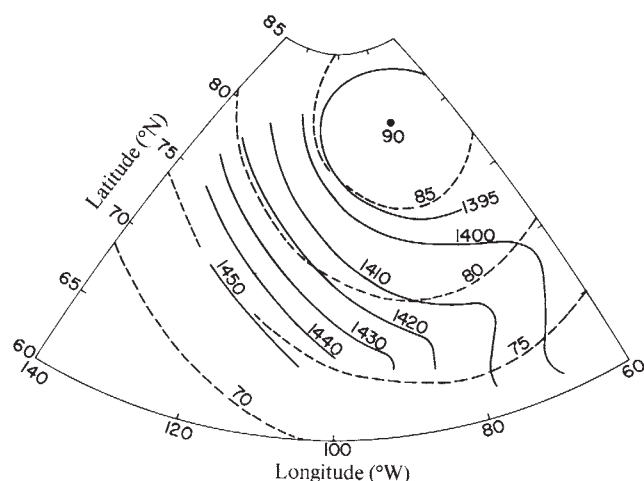


Fig. 6 A plot in geographic coordinates containing contours (continuous curves) published by Bradley⁴ showing the average height of the 850 mbar surface in July during the years 1964-72. ---, Positions of lines of constant invariant latitude; the invariant magnetic pole is marked 90.

magnetic noon. Such observations are consistent with predictions made using observed¹⁷ and theoretical¹⁸ models of the magnetosphere; these models show that the high latitude areas surrounding the invariant latitude poles are strongly shielded against the incursion of solar particles.

Figure 6 contains contours, published by Bradley⁴, showing the average height of the 850 mbar surface in July over the Canadian Arctic region during the years 1964-72. These contours are compared in Fig. 6 with curves representing the 70°, 75°, 80° and 85° invariant latitude lines. Over most of the area the meteorological contours are nearly parallel to the constant-invariant-latitude lines; the south-eastern sector is, however, as Bradley pointed out, dominated by a ridge of high pressure. This ridge is seen to occur at invariant latitudes between 76° and 79°, precisely where solar wind particles penetrate into the atmosphere most easily. The magnitude of the ridge may be seen by following the 70° W geographic meridian towards the invariant pole; the pressure is greatest at a latitude of about 78° invariant.

The evidence contained in Fig. 6 does not, of course, demonstrate conclusively that the meteorological pressure in the region investigated is affected by energetic particles, but it certainly suggests that further analyses of this kind should be undertaken. The incident particle energy and the regions to which it penetrates will vary during the solar cycle, with season and as a function of the magnetic activity prevailing, so it should not be expected that meteorological effects which may be associated with particles will remain constant. Bradley has observed that the pressures over the Canadian Arctic were very different in 1955-63 from those in 1964-72.

As long ago as 1948, Duell and Duell¹⁹ noticed that, under certain conditions, geomagnetically disturbed days were followed by pressure rises of about 3 mbar in the Greenland-Iceland area. It seems reasonable to suggest now that this behaviour may be a consequence of the incursion of energetic particles which arrive at an invariant latitude of about 75° during disturbed conditions. (Frank¹⁶ showed that particles enter at 79° during quiet conditions, but at a latitude lower by about 4° invariant during disturbed conditions.) The 75° invariant latitude line passes through a point 40° W, 69° N (that is, through the centre of southern Greenland), within the region where pressure increases are observed. It is interesting to note in this connection that climatic fluctuations which are global or hemispheric in extent are most pronounced in high northern latitudes, particularly around the north Atlantic.

Implications for Weather Prediction

The results of several recent meteorological studies have been used to show that changes which occur in the solar radiation during the sunspot cycle appear to be accompanied by secular meteorological variations that affect the temperature and pressure in the northern hemisphere. The growing season in Scotland, for example, seems to be highly dependent on the sunspot cycle and it seems reasonable to suggest that investigations of how to incorporate solar activity into long-range weather forecasts issued for agricultural planning purposes, for example, may yield significant returns for the effort involved.

The effect of solar activity changes on rainfall may be very indirect and therefore less distinct, although it seems that very reduced winter rainfall occurs over parts of the north Africa-north India region following periods of high solar activity. This result may seem more significant when related to the fact that the driest winters at Kew occur during years of low solar activity.

The physical mechanisms by which solar radiation and the weather are connected are undoubtedly complex and, at the present time, quite unknown. It has, however, been concluded that solar wind particles entering the atmosphere at invariant latitudes between 75° and 79° may be associated with meteorological pressure changes. If further work shows that this is indeed the case, a major clue in the field of solar-weather relationships will have been established. It should be borne in mind in our present state of ignorance that meteorological effects associated with energetic particles may not be produced directly; variations in the Joule heating produced by the auroral electrojet or other current systems (in the ionosphere or even in the Earth) which are affected by solar changes may be involved. It has already been established¹²⁻¹⁴ that the neutral atmosphere above 100 km is very sensitive to geomagnetic disturbances and the normal thermospheric winds, the existence of which was first postulated²⁰ in 1965, will be drastically altered²¹ during disturbed conditions.

The sunspot number is, however, a relatively crude index of solar activity which should be abandoned, in future work on solar-weather relationships, in favour of a more meaningful index such as the velocity of the solar wind or even the 10.7 cm solar flux.

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Regional Distribution of Opiate Receptor Binding in Monkey and Human Brain

MICHAEL J. KUCHAR, CANDACE B. PERT & SOLOMON H. SNYDER

Departments of Pharmacology and Experimental Therapeutics, and Psychiatry and the Behavioral Sciences, Johns Hopkins University School of Medicine, Baltimore, Maryland

The frequency of opiate receptors varies dramatically throughout the human and monkey brain. The authors describe a relationship between the distribution of opiate receptors and of neurotransmitters.

BASED on the prediction of Goldstein *et al.*¹ that opiate receptor interactions should be stereospecific, we recently demonstrated binding to a pharmacologically relevant opiate receptor in the brains of several mammalian species^{2,3}, a finding which has been independently⁴ and subsequently⁵ confirmed. Opiate receptor binding seems to be restricted to membrane fractions in nervous tissue², and has been demonstrated in a large number of vertebrate species, but cannot be detected in invertebrates (Pert, Aposhian and Snyder, in preparation). Binding is sensitive to low concentrations of phospholipase A and proteolytic enzymes such as trypsin and chymotrypsin⁶. Although corresponding opiate agonists and antagonists do not differ in their affinities for the opiate receptor *in vitro*³, antagonists are markedly more potent in increasing the number of opiate receptor binding sites after administration *in vivo*, a finding which is unrelated to differential drug metabolism or penetration into the brain⁷.

To elucidate how opiate receptor interactions mediate pharmacological effects, and to identify a possible naturally occurring substrate for the opiate receptor, we investigated opiate receptor binding in different regions of the rat brain². Binding was highest in the corpus striatum, undetectable in the cerebellum and intermediate in other areas, which parallels the regional distribution of acetylcholine. To evaluate regional localisation of the opiate receptor in greater detail, we have measured opiate receptor binding in many regions of monkey and human brain. In addition we have evaluated the influence of lesions of neurotransmitter-specific tracts on opiate receptor binding in the rat brain.

Five rhesus monkeys (*Macaca mulatta*) were injected intraperitoneally with 30–45 mg kg⁻¹ of sodium pentobarbital, a drug which has no influence on opiate receptor binding after *in vivo* or *in vitro* administration in high doses⁷, and were decapitated 2 h later. The skull was opened with an autopsy saw and the brain transferred to ice-cold 0.9% NaCl within 5 min. Two human brains were obtained from male subjects aged fifty-seven and forty-eight yr, 10 to 14 h after their deaths, from gunshot wounds to the thorax and acute myocardial infarction respectively. The human corpses were maintained at 4° C beginning 1–2 h after death until the brain was removed. To examine the effect of this treatment on opiate receptor binding, a group of four rats was killed, maintained first at 25° C for 2 h and then at 4° C for 4 h before assay. The treatment produced a 35–40% decrease in stereospecific opiate receptor binding when compared with

the brains of rats killed and assayed immediately, but the regional variation between cortex and striatum² was preserved by this treatment. After dissection, brain regions were homogenised in 150 volumes of 0.05 M Tris buffer and protein concentrations were determined⁸. Stereospecific opiate receptor binding was assayed on the fresh homogenates by a modification of previously described methods^{2,3}. Homogenates (1.9 ml) were incubated in triplicate in the dark for 30 min at 25° C in the presence of 100 nM levorphanol or dextrorphan after the addition of ³H-dihydromorphine (1–2 nM final concentration), filtered and washed with two 5 ml portions of ice-cold Tris buffer. Binding values reported were linear with tissue protein.

Radio frequency lesions were used to destroy the medial septum, locus coeruleus and midbrain raphe nuclei of chloral hydrate anaesthetised rats. This treatment results in an extensive degeneration of cholinergic, noradrenergic and serotonergic tracts respectively in various forebrain regions. Degeneration appears to be complete at approximately 7 d after lesioning^{9–12}.

Regional Distribution of ³H-Dihydromorphine Binding

Opiate receptor binding varied dramatically throughout the monkey brain, with more than a thirty-fold variation (Table 1). The amygdala contained the greatest amount of binding, with the anterior portion displaying almost twice as much binding as the posterior amygdala. Binding in the periaqueductal area of the midbrain was about the same as that of the posterior amygdala. The hypothalamus and medial thalamus, the next highest areas, displayed only about 40% of the binding of the anterior amygdala. No regional variations were detected within the hypothalamus, while the medial thalamus possessed about three times as much binding as the lateral thalamus. In the head of the caudate, receptor binding was about 80% of that in hypothalamus and medial thalamus, double that of the body and tail of the caudate and 1.7 times binding in the putamen. Levels of binding in the putamen were similar to those of the hippocampus, the superior colliculi, interpeduncular nucleus area, frontal pole and superior temporal gyrus of the cerebral cortex.

Within the cerebral cortex there were marked regional variations. Thus binding in the precentral gyrus, postcentral gyrus and occipital pole was less than a quarter of that in the frontal cortex. Receptor binding was very low or not detectable in white matter areas such as the corpus callosum, corona radiata, fornix and optic chiasm.

As had been observed before², opiate receptor binding was quite low in the cortex and dentate nucleus of the cerebellum and in the lower brain stem and spinal cord. Within the spinal cord, binding was greater in grey than in white matter.

For some brain regions, several concentrations of ³H-dihydromorphine were examined. Kinetic analysis of these data indicated that regional differences in stereo-

specific ^3H -dihydromorphine binding reflected variations in total number of receptor sites (V_{max}) rather than in affinity (K_m).

Regional variations in opiate receptor binding in the human grossly resembled those in the monkey. The greatest amount of binding was in the amygdala and the thalamus. The thalamic sample analysed represented predominantly medial portions of this brain region. The concentration of opiate receptors was next highest in the human in the head of the caudate and the hypothalamus. In the human as in the monkey, the periaqueductal area of the midbrain and the putamen displayed high levels of binding, while negligible

binding was detected in the cerebellar cortex. Also as in the monkey, binding varied within the human cerebral hemisphere, with five times as much binding in the frontal pole as in the occipital pole and negligible binding in the corona radiata.

Comparisons with Neurotransmitter Distribution

Most brain biochemicals are distributed rather uniformly throughout the brain. The striking heterogeneity of opiate receptor distribution is reminiscent of the large regional dif-

Table 1 Opiate Receptor Binding in Regions of Monkey and Human Brain; Choline Acetyltransferase, GABA and Tyrosine Hydroxylase Distribution in Monkey Brain

Region	Human		<i>Macaca mulatta</i>		<i>Macaca mulatta</i>	
	Stereospecific dihydromorphine binding (fmol per mg protein)		Stereospecific dihydromorphine binding (fmol per mg protein)		Choline acetyltransferase (nmol per 30 min per mg protein)	Tyrosine hydroxylase* (nmol dopa formed per h per g frozen tissue)
Cerebral hemispheres						
Frontal pole	14.6	(1)	11.9 ± 1.4	(4)	2.4	18.8 †
Superior temporal gyrus			10.8 ± 2.5	(3)	2.7	
Middle temporal gyrus			7.1	(1)		
Inferior temporal gyrus			6.0	(1)		
Precentral gyrus			3.4	(2)	2.6	18.8 ‡
Postcentral gyrus			2.8	(2)	2.6	< 0.5 ‡
Occipital pole	2.8	(2)	2.3 ± 0.5	(4)	1.8	22.4 †
White matter areas						
Corpus callosum			< 2	(2)	0.8	
Corona radiata	< 2	(1)	< 2	(2)		
Anterior commissure			5.4	(2)		
Fornix			< 2	(2)	4.2	
Optic chiasm			< 2	(2)	0.6	
Limbic cortex						
Anterior amygdala			65.1 ± 21	(4)		
Posterior amygdala			34.1 ± 4.2	(4)		
Amygdala	46.4 ± 3.6	(4)				
Hippocampus	12.4	(1)	12.5 ± 2.2	(4)	7.2	
Hypothalamus						
Medial hypothalamus	27.8	(1)	24.2 ± 3.2	(4)		
Anterior hypothalamus	27.9	(1)	24.3 ± 3.7	(3)	4.6	
Posterior hypothalamus			24.7 ± 1.4	(3)	4.0	
Hypothalamus	21.8	(1)	23.2	(1)		59.6
Mammillary bodies	5.1	(1)	5.0	(1)		8.2
Thalamus						
Medial thalamus			24.6 ± 1.6	(3)		26.3
Lateral thalamus			7.8	(2)		22.5
Thalamus	48.5 ± 9.0	(4)				< 0.2
Extrapyramidal areas						
Caudate nucleus (head)	38.0, 23.9	(2)	19.4 ± 2.3	(4)	25.4	28.4 §
Caudate nucleus (body)			9.0 ± 1.2	(3)		108.8 §
Caudate nucleus (tail)			8.9 ± 3.0	(3)		
Putamen	11.2, 27.7	(2)	11.7 ± 1.9	(6)	38.1	29.5
Globus pallidus	6.7	(1)	7.7	(2)		85.2
Internal capsule			5.4	(2)	6.1	111.6
Midbrain						
Superior colliculi			10.6 ± 2.0	(3)	8.8	37.5
Inferior colliculi			6.7 ± 0.70	(3)	6.2	44.9
Interpeduncular nucleus area			13.7 ± 1.5	(4)	32.6	< 0.8
Raphe area			8.2	(2)		< 0.8
Periaqueductal grey	21.2	(2)	31.1 ± 4.6	(4)	5.5	40.2
Cerebellum-lower brain stem						
Pons (ventral)			1.4	(2)	5.4	
Cerebellar cortex	< 2	(2)	< 2	(2)	0.4	16.7
Dentate nucleus			1.9	(2)	1.3	42.1
Floor of fourth ventricle			6.3 ± 1.3	(3)	6.0	< 0.1
Pyramidal tract			3.0	(2)	1.1	
Lower medulla			5.8	(2)	5.6	
Spinal cord (thoracic)						
Dorsal column (white)			3.1	(2)		
Lateral cord (white)			3.3	(1)	1.4	
Grey matter			8.8	(2)	5.4	

* From Côté and Fahn^{15,16}. † Cortex, unspecified. ‡ Motor cortex. § Caudate, unspecified.

Opiate receptor binding was assayed as described in the text. Values represent the mean ± s.e.m. followed in parentheses by the number of determinations. Each determination was calculated from triplicate incubations with 1 nM ^3H -dihydromorphine in the presence of 100 nM levorphanol or dextrorphan. Choline acetyltransferase values are the average of two determinations, each performed in duplicate.

ferences found for concentrations of neurotransmitters and their biosynthetic enzymes.

Because initial regional studies in the rat brain² had suggested an association with cholinergic neuronal systems, samples from monkey brain regions were also assayed for choline acetyltransferase. Both similarities and differences existed in the regional distributions of choline acetyltransferase and the opiate receptor (Table 1). Thus while both choline acetyltransferase activity and opiate receptor binding values in the monkey were high in the caudate, the putamen contained the highest choline acetyltransferase values but only moderate opiate receptor binding. The periaqueductal area of the midbrain, which contained the second highest opiate receptor values, possessed only moderate choline acetyltransferase activity.

Regional variations in γ -aminobutyric acid (GABA) content in rhesus monkey brain¹⁵ also displayed both similarities to and differences from that of the opiate receptor (Table 1). Thus while the hypothalamus was rich in GABA content and opiate receptor binding, the globus pallidus, which contained one of the highest GABA concentrations in the brain, was relatively low in opiate receptor binding. The occipital cortex contained more GABA than the frontal cortex, while their relationship was reversed for opiate receptor binding. Although GABA levels are known to rise rapidly after death, levels reported for rhesus monkey brain were closely similar to those for glutamic acid decarboxylase¹⁶, the GABA synthesizing enzyme in this species suggesting that these levels reflect the occurrence of GABA neurones.

Regional differences in tyrosine hydroxylase activity¹⁷ should provide some index of the relative density of dopamine and norepinephrine neurones. Like the opiate receptor, high tyrosine hydroxylase activity occurred in the caudate. Although the putamen was as active in tyrosine hydroxylase as the caudate, it contained only half the opiate receptor binding of the caudate. The hypothalamus and periaqueductal grey were relatively rich in tyrosine hydroxylase and also contained high levels of opiate receptor binding. While the medial thalamus contained much more opiate receptor binding than the lateral thalamus, the reverse situation occurred for tyrosine hydroxylase activity. Within the corpus striatum the relative amounts of tyrosine hydroxylase activity in caudate nucleus and globus pallidus were similar to the relative proportions of opiate receptor binding.

Although we are not aware of a detailed study of the regional distribution of serotonin, or its biosynthetic enzyme tryptophan hydroxylase, in rhesus monkey brain, data from cat brain may provide relevant information¹⁸. The highest tryptophan hydroxylase activity in cat brain was observed

in the caudate, which was about 1.6 times that of the hypothalamus and about twice that of the amygdala. Thus for both tryptophan hydroxylase and opiate receptor binding, these three regions were the highest in the brain, although their relationship was reversed, since for the opiate receptor the amygdala contains substantially more binding than the caudate. The midbrain raphe area, which contains the cell bodies of serotonin neurones in the brain, is, however, one of the richest areas in tryptophan hydroxylase activity but is quite low in opiate receptor binding.

Transmitter-specific Lesions

Whereas the regional distribution of opiate receptor binding does not correlate perfectly with that of any known neurotransmitter or its enzymes, it is conceivable that opiate receptor sites are contained within a single neurotransmitter-specific tract in such a way that they fail to parallel the biochemical markers we considered. Accordingly, lesions were performed on specific cholinergic, noradrenergic and serotonergic tracts. Lesions of the septal area of the rat brain destroy cholinergic projections to the hippocampus, as is shown by a profound reduction in hippocampal levels of acetylcholine, choline acetyltransferase and high affinity choline uptake¹¹ (Table 2). At intervals of 4, 7, 15 and 49 d after such lesions there was no alteration in opiate receptor binding in the hippocampus. Electrolytic lesions of the locus coeruleus destroyed ascending noradrenergic projections causing a 70% decrease in endogenous noradrenaline in the ipsilateral cerebral cortex¹². ³H-Noradrenaline uptake is reduced in brain regions where noradrenergic neurones have been destroyed^{13,14}. There was no associated change in opiate receptor binding 21 d after placement of the lesion (Table 2). Electrolytic destruction of the midbrain raphe nuclei is followed by a 70–80% decrease in high affinity serotonin uptake, endogenous serotonin and tryptophan hydroxylase activity in the forebrain^{9,10}, but no change in opiate receptor binding was apparent in the forebrain 15 d after placement of the lesions (Table 2).

The failure of lesions of noradrenergic, serotonergic and cholinergic pathways to alter opiate receptor binding in areas in which terminals of these pathways were destroyed indicates that the opiate receptor is not a unique component of axons or nerve endings of any one of these neuronal tracts. Because considerable amounts of opiate receptor binding are detected in forebrain areas which lack serotonergic and catecholaminergic cell bodies, it is unlikely that the opiate receptor is associated exclusively with cell bodies of these neuronal systems. We cannot exclude the possibility that opiate receptor sites are associated with post-synaptic ele-

Table 2 Effect of Destruction of Noradrenergic, Serotonergic and Cholinergic Tracts on Opiate Receptor Binding

Neurotransmitter	Lesion area	Assay area	Days post-lesion	Assay (control/lesion)	
				NA uptake	Opiate receptor (fmol per mg protein)
Noradrenaline (NA)	Locus coeruleus	Ipsilateral cerebral cortex	21	0.88/0.37	4.0 ± 0.2/4.1 ± 0.2
Serotonin (5-HT)	Midbrain raphe nuclei	Forebrain	15	5-HT uptake	
				3.4/1.0	1.9 ± 0.3/2.2 ± 0.3
Acetylcholine	Medial septum	Hippocampus		Choline acetyltransferase	
			4	3.4/0.68	16.5 ± 2.0/17.4 ± 2.2
			7	—	21.3 ± 2.1/22.2 ± 1.9
			15	3.9/0.65	17.8 ± 2.8/19.0 ± 1.9
			49	—	18.6 ± 0.9/17 ± 1.2

Lesioned animals were prepared as described in the text. Synaptosomal uptake and choline acetyltransferase assays were performed as previously described^{9,11}. Placement of lesions was verified by histological examination in all cases, and uptake and enzyme assays were used with some animals from each group to further verify tract degeneration. Incubation media concentrations of NA and 5-HT were 0.1 μ M and 10 nM respectively. Uptake results are mean values from three animals and are expressed as pmol per 4 min per 1.8 mg protein. Choline acetyltransferase activity is expressed as μ mol h⁻¹ g⁻¹. Opiate receptor binding was assayed as described in the text. Values represent the mean ± s.e.m. of five experimental determinations and their five controls. Each determination was calculated from triplicate incubations with 1 nM ³H-dihydromorphine in the presence of 100 nM levorphanol or dextrorphan.

ments of these tracts, or with more than one neurotransmitter system.

Does the relative density of opiate receptor sites in particular brain areas reflect their role in the pharmacological actions of opiates? In experiments in which morphine was injected stereotaxically into numerous sites of rhesus monkey brain, the analgesic response was limited to the mesencephalic and ventral diencephalic areas surrounding the third and fourth ventricles¹⁹. Analogous experiments with rodents also suggest that these areas are critical for eliciting analgesia^{20,21}. Following application of naloxone crystals to several sites in the rat brain, abstinence signs in opiate-addicted animals were most frequently elicited in the medial thalamus and in medial areas of the diencephalic-mesencephalic junctures²², which are among the richest in opiate receptor binding.

Interestingly, these regions and others rich in opiate receptor binding are all portions of the limbic system²³. In our dissection, the head of the caudate, with more than twice the receptor binding of other parts of the corpus striatum, includes the nucleus accumbens, a component of the limbic system. The frontal and superior temporal cerebral cortices, more closely associated with the limbic system than other areas of the cerebral cortex, contain four to five times more receptor binding than other parts of the cerebral cortex. Opiate receptor binding is, however, not uniformly high throughout the limbic system, since the mammillary bodies and hippocampus respectively possess low and moderate binding activity.

What is the significance of the dramatic regional variations in the density of opiate receptor sites? If these binding sites are receptors for some naturally-occurring brain constituent, one might expect such a compound to have a similar regional localisation. The absence of a close correlation with known neurotransmitters and the failure of specific lesions to alter opiate receptor binding argues against an exclusive association with cholinergic, serotonergic or noradrenergic neurones. It is, of course, conceivable that the opiate receptor is unrelated to any neurotransmitter system, known or unknown. Still, the striking regional variations indicate that it must be associated with some brain component possessing a

'limbic' distribution, whose identification would shed light on the mechanism of opiate action.

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LETTERS TO NATURE

PHYSICAL SCIENCES

Nature of the Lower D Region of the Ionosphere

IONS in concentrations of about 10^2 to 10^3 cm⁻³ have been measured during day and night by means of rockets in the mesosphere. The resulting data have been used by Bragin *et al.*^{1,2} as the basis of two hypotheses: (1) that the high concentrations of ions below 90 to 80 km is produced during both day and night by the ionising agent (the flow of particles with medium energies) and (2) that the D region of the ionosphere below 80 km is formed in daytime because of the photodetachment of electrons from negative ions by photons of sunlight with energies below 10 eV. No assumptions have been made about the nature of the negative ions involved. The data for the actual aeronomic reactions used were as follows: (1) the rate coefficient of the detachment of electrons from the negative ions was assumed to be 10^{-2} to 5×10^{-1} s⁻¹; (2)

the rate coefficient of the detachment of electrons from negative ions by processes other than photodetachment was assumed to be of the order of 1 s⁻¹ at 60 km, and 10^{-1} to 10^{-2} s⁻¹ at 80 km; (3) the rate of attachment of electrons to the molecular oxygen was assumed to be 1.5×10^{-30} [O₂]² cm³ s⁻¹, where [O₂] is the concentration of the molecular oxygen corresponding to the standard atmosphere.

Thomas *et al.*^{3,4} have solved the continuity equations numerically using up to date data about the molecular ionic structure and the aeronomic reactions in the D region of the ionosphere. The profiles of the concentration of electrons obtained in the daytime and at night confirmed the hypothesis that the daytime D region of the ionosphere below 80 km may be formed by photodetachment of electrons. The larger ions, probably hydrates, have not been taken into account in the calculations of Bragin *et al.* and Thomas *et al.*, which is a considerable simplification of the problem, as Thomas *et al.* have pointed out. Another short-coming of that work is that the results of direct measurements in the D region carried out by Soviet workers are omitted.

The primary information about the nature of the negative ions in the mesosphere above 70 km is given by mass spectrometric measurements^{5,6}. Unfortunately, not all the ions contributing to the total ionic current in the mass spectrometer were classified into ion species, and the identifications of the larger ions are not certain. Thus, the nature of the principal ions, both positive and negative, in the D region below 80 km is still not known. On the other hand, it seems that the negative and positive ions below 70 to 80 km are larger cluster ions. Thus, the next step in the development of the hypotheses of formation of the daytime D region of the ionosphere will be the introduction of the larger ions into the equilibrium equations. The ideal would be to obtain direct mass spectrometric measurements of the concentration and constitution of the larger ions. But that is a task for the future.

There are other experimental data which illustrate the action of the photodetachment processes at 60 to 80 km. The dependence of the electrical conductivity σ , measured by day and night flights of rockets in the autumn of 1971, on the height in the atmosphere is shown by Bragin *et al.*⁷ The method of dissipation of electrical charge was used in these measurements; σ was determined by the time constant of the charge dissipation of a charged sphere placed in front of the nozzle of the rocket, and the result obtained showed that at heights of 60 to 80 km the conductivity at night was 10^{-11} to $10^{-10} \Omega^{-1} \text{ m}^{-1}$, and in the day 10^{-10} to $10^{-9} \Omega^{-1} \text{ m}^{-1}$. The diurnal variation of the total concentration of charged particles $[n^{\pm}]$ was not so great at these heights⁸. The concentration of charged particles $[n^{\pm}]$ was determined by measuring the saturation current I_s produced by small ions entering from the atmosphere, together with the airflow running to the rocket, into a hollow cylinder with a cross section F . The aerodynamics of the cylinder were adjusted so that in the calculations the formula

$$[n^{\pm}] = I_s / e v_r F$$

could be used, with v_r the speed of the rocket. It may be concluded that the sharp increase of the conductivity $\sigma = e[n^{\pm}]\mu_{\text{eff}}$ during the day was caused mostly by the increase of the daytime effective mobility μ_{eff} . This may happen in the atmosphere at sunrise, because of the appearance of light charged particles, probably electrons. This effect was observed down to 20 km. Thus, the daytime light particles below 80 km are not necessarily electrons.

The data relevant to hypotheses about the D region of the ionosphere can now be supplemented by the results of rocket measurements of aerosols in the atmosphere up to 90 km by means of optical counters⁹. The conductivity σ was measured simultaneously. The maxima of concentration of aerosols were mostly observed when the electrical conductivity was at minima. Thus, the possibility of regulating the number of charged particles in the D region of the ionosphere by aerosols may be taken into consideration.

The maximal values of $E_a \approx 10 \text{ V m}^{-1}$ were measured at about 60 km by the first rocket measurements of the vertical component of the electrical field up to a height of 90 km (ref. 10). The separation of charged particles, including aerosols, by electrical fields must be taken into account in the theory of the D region.

The collected data do not contradict the idea of the formation of the lower part of the D region of the ionosphere by the photodetachment of electrons from the negative ions or other particles (for example, from charged aerosols). But the further development of a scientific theory for the D region of the ionosphere will not be forced by estimations like those considered in refs 1 and 2 or by mathematisation or mechanisation of the equilibrium equa-

tions (a method which now fascinates many people), without taking into account information about the constitution of the larger ions and aerosol particles and the electrical fields in the lower atmosphere.

YU. A. BRAGIN

Novosibirsk State University,
Novosibirsk 90, USSR

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Production of Ice Crystals in Slightly Supercooled Cumulus

PERHAPS the most puzzling observation in cloud physics reveals that some slightly supercooled cumulus clouds, with summit temperatures only a few degrees below 0°C , contain high concentrations of ice crystals—two to four orders of magnitude greater than the concentrations of ice nuclei measured in clear air at the same level. According to a series of careful and detailed aircraft observations and measurements by Mossop and his colleagues, such high concentrations of crystals occur only in aged cumulus that has been in existence for some time and acquired widths of several kilometres but are absent from newly rising cloud towers. They are also absent from slightly supercooled stratocumulus clouds. Mossop *et al.*^{1,2} report small ice crystals in concentrations of 10 to 100 l^{-1} in clouds with summit temperatures as high as -8°C but always in association with rimed ice pellets of diameter of order 1 mm in concentrations of order 1 l^{-1} and with large droplets of $r > 250 \mu\text{m}$ in concentrations of order 100 m^{-3} . The concentrations of ice nuclei active at -8°C are below those that can be reliably measured by conventional techniques but probably do not exceed 1 m^{-3} .

This large discrepancy between the concentrations of ice crystals and of ice nuclei suggests that some process of ice crystal multiplication must be active in the cloud. The most plausible suggestion is that small ice splinters are ejected during the accretion and freezing of supercooled cloud droplets on small ice pellets which would be consistent with the fact that high concentrations of ice crystals occur only in the company of relatively large rimed ice particles. Although ice splinters are sometimes observed to be ejected during the freezing of drops of $r > 30 \mu\text{m}$, a careful series of laboratory experiments by S. C. Mossop, J. L. Brownscombe and G. J. Collins (to be published) and by M. Bader *et al.* (to be published) have found the rate of splinter production during the growth of riming elements to be much too low to account for the high concentrations of ice crystals in clouds reported by Mossop *et al.* if the rimed particles are assumed to originate at much the same level as the crystals are observed.

The picture changes substantially, however, once one realizes that the large rimed pellets can hardly have grown

in situ but must have originated at a higher level and grown by accretion as they fell towards the level of observation. In other words, we postulate that, in an earlier, active stage of its development, the cloud top penetrated to higher and colder levels and subsequently subsided to a lower level by the time it was penetrated by the observing aircraft. For example, a rising cumulus tower reaching to only 1 km above the observation level would attain a temperature some 8° C lower and activate a nucleus concentration two orders of magnitude greater than that to be expected at the observing level. Ice particles originating at these higher levels would then grow by the accretion of cloud droplets and by shedding ice splinters during their fall through the subsiding cloud tower and might largely account for the ice crystals appearing at and below the observing level.

I shall suppose that at some earlier stage in its history the cloud reached a maximum height z_m above the observing level $z=0$. At some intermediate level, z , a concentration $N(z)$ of ice nuclei is activated and forms ice particles that continue to grow by vapour deposition and accretion of droplets and finally arrive at the observation level with radius $R(z,0)$. The concentration of ice splinters produced at the observation level by ice particles originating at levels between z and $z+dz$ is then

$$\Delta S(z,0) = E N(z) \pi R^2(z,0) n f dz \quad (1)$$

where E is the collection efficiency of the ice pellet for cloud droplets of concentration n , and f is the fraction of collected droplets that produces an ice splinter.

$$\text{But } R(z,0) = E w z / 4 \rho \quad (2)$$

where w is the concentration of liquid water and ρ is the density of the rimed pellet.

The concentration of splinters at the observing level due to ice pellets arising at all levels up to $z=z_m$ is then

$$S = (E^3 \pi w^2 n f / 16 \rho^2) \int_0^{z_m} N(z) z^2 dz \quad (3)$$

Since measurements indicate that, on average, ice nucleus concentrations increase ten-fold for every 4° C fall in temperature, corresponding to an increase in height of about 500 m in the cloud, it follows that

$$N = N_0 \exp(\alpha z) \quad (4)$$

where N_0 is the concentration active at the observing level and $\alpha = 4.6 \times 10^{-5} \text{ cm}^{-1}$.

Equation (3) then becomes

$$S/N_0 = (E^3 \pi w^2 n f / 16 \rho^2) [(\exp(\alpha z_m)/\alpha)(z^2 - 2z_m/\alpha + 2/\alpha^2) - 2/\alpha^3] \quad (5)$$

Putting $E=0.8$, $w=1 \text{ g m}^{-3}$, $n=100 \text{ cm}^{-3}$, $\rho=0.3 \text{ g cm}^{-3}$ gives the values (Table 1) of the ice-crystal multiplication factor $S/N_0 f$ for various values of z_m and the corresponding values for the radii of the largest rimed pellets crossing the observation level.

The times, t , taken for the pellets to reach these sizes are given by

$$t = (4\rho/Ew) \int dR/V \quad (6)$$

where their fall velocity³

$$V = 300 + 100 \log 2R \quad (7)$$

V being in cm s^{-1} and R in cm. They are shown on the last line of Table 1.

Thus if, on average, an ice splinter were shed by a rimed pellet for the accretion of every 1,000 droplets, that is, $f=10^{-3}$, equation (5) predicts that ice pellets falling from up to 1 km

above the observing level would produce a concentration of ice crystals some 1,000 times greater than the concentration of ice nuclei initially active at that level. Assuming the latter quantity at the -8°C level to be $N_0=1 \text{ m}^{-3}$, the calculated ice crystal concentration at that level is 1 l^{-1} and somewhat higher than this at levels closer to the 0°C isotherm.

Table 1 Calculated Parameters for Ice Crystals

	z_m (km)			
	1.0	1.25	1.5	2.0
$S/N_0 f$	1.6×10^6	8.3×10^6	4×10^7	10^9
R_m (mm)	0.67	0.83	1.0	1.33
t (s)	540	630	720	

Further multiplication of the ice crystal population would seem to require a second stage in the growth of the cloud unless the first tower ascends to considerably more than 1 km above the observing level before subsiding. Table 1 shows that such an ascent to 1.5 km would produce a multiplication factor of 4,000. Alternatively one may envisage a second stage in which the crystals produced between, say, the 0° and -8°C levels during the first stage are caught up in a newly rising tower, the summit of which also reaches 1 km above the -8°C level. Assuming that the ice crystals become uniformly mixed through the volume of the new thermal during its ascent, reach a concentration of $N=1 \text{ l}^{-1}$, and that these grow by accretion, produce splinters and eventually fall out through the -8°C level, they would produce there ice crystals in a concentration

$$S' = (E^3 \pi w^2 n f N / 16 \rho^2) \int_0^{z=1 \text{ km}} z^2 dz = E^3 \pi w^2 n f N z^3 / 48 \rho^2 \quad (8)$$

With $N=1 \text{ l}^{-1}$, $n=10^2 \text{ cm}^{-3}$, $w=10^{-6} \text{ g cm}^{-3}$, $\rho=0.3 \text{ g cm}^{-3}$, $f=10^{-3}$ and $z=1 \text{ km}$, this yields

$$S' = 40 \text{ l}^{-1}$$

so that the crystal concentration is increased by a factor of 40 in this second stage to give an overall magnification factor of $S'/N_0 = 4 \times 10^4$.

If the rate of splinter production were to be an order of magnitude smaller, that is, $f=10^{-4}$, the overall magnification factor would be only 400 and too small to account for the highest concentrations of crystals reported by Mossop *et al.*

In summary, it seems possible to explain the production of high concentrations of ice crystals in slightly supercooled cumulus clouds on the following suppositions: (i) the growth of rimed pellets is accompanied by the shedding of ice splinters at an average rate of at least one splinter per thousand accreted droplets; (ii) the rimed pellets grow and fall out from cloud towers that stretch at least 1 km above the observation level; (iii) the crystals produced below the observation level are caught up in a second rising tower which ascends to at least the same height as the first and releases a second batch of rimed pellets that enhance further the ice crystal concentration at the observation level; (iv) as the growth and fall of each batch of ice pellets take about 10 min, the lifetime of the cloud as a whole, which must also allow for the ascent of the towers, must be an hour or more. Such clouds are also likely to contain large droplets of the size reported by Mossop *et al.*

Since the growth of rimed pellets in successive towers is peculiar to cumulus clouds one would not expect the proposed mechanism to work in stratocumulus which indeed does not contain high concentrations of crystals. The efficiency of the suggested mechanism is rather sensitive to the adopted value of f , the rate of splinter production. It has to be admitted that in careful experiments carried out in the laboratory by

Mossop *et al.* (to be published) in which small droplets were allowed to impinge and build up a rime deposit on a whirling cylinder, only one splinter was observed per 30,000 droplets of radius $< 20 \mu\text{m}$ and one per 10^6 droplets of $r = 6 \mu\text{m}$.

It is, however, possible that very small splinters may have evaporated and escaped detection. In an experiment designed to detect such small splinters in the supersaturated atmosphere of a diffusion cloud chamber, Bader *et al.* (to be published) find one splinter per 2,000 impacting droplets of radius $27 \mu\text{m}$. Since the average droplet radius in natural clouds is only about $10 \mu\text{m}$, the rate of splinter production could well be below that observed by Bader *et al.* and so below the value of $f = 10^{-3}$. The rime target in the experiment was, however, stationary and so could not simulate the ventilation of a freely falling pellet in a natural cloud.

I cannot claim to have produced a completely convincing explanation of the highest observed concentrations of ice crystals in slightly supercooled cumulus, but the mechanism suggested reduces the discrepancy between crystal and nucleus concentrations from several to one order of magnitude. It is hoped that these ideas will stimulate observers to make careful observations of the life history of the cloud, in particular of the maximum height of towers relative to the observing level, and encourage the design of experiments to detect very low rates of production of very small splinters from rimed elements growing under realistic atmospheric conditions.

B. J. MASON

Meteorological Office,
Bracknell, Berkshire

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Radiocarbon Dating from Ogof-yr-Ychen, a New Pleistocene Site in West Wales

THERE has recently been renewed discussion concerning the extent of the Weichselian Irish Sea Glacier¹⁻³, and several authors have stressed the difficulty of determining its southern margin in Wales on the basis of geomorphological and stratigraphic criteria alone. Also, in South Wales there is no known reliable interglacial or interstadial palaeobotanical site which can be used for the dating of drift sequences. Archaeological investigations in favourable localities may assist greatly in this debate, as well as extending knowledge of Pleistocene faunas and of human Palaeolithic occupation on the South Wales lowlands^{4,5}. Already it is suggested that substantial parts of Pembrokeshire and the Gower Peninsula were unaffected by ice during the last glaciation³, allowing the possibility of more or less continuous human Palaeolithic occupation of some coastal sites. The radiocarbon age determination for the "Red Lady" of Paviland ($18,460 \pm 340$ yr BP, BM374)⁶ supports this view, indicating that there was Proto-Solutrean occupation of cave sites in Gower close to the peak of the last glaciation. This was followed by a Creswellian occupation, probably after 15,000 yr BP^{7,8}. In Pembrokeshire the most promising Upper Palaeolithic and Mesolithic finds have been made during a series of excavations on Caldey Island since 1911 (refs 9 and 10).

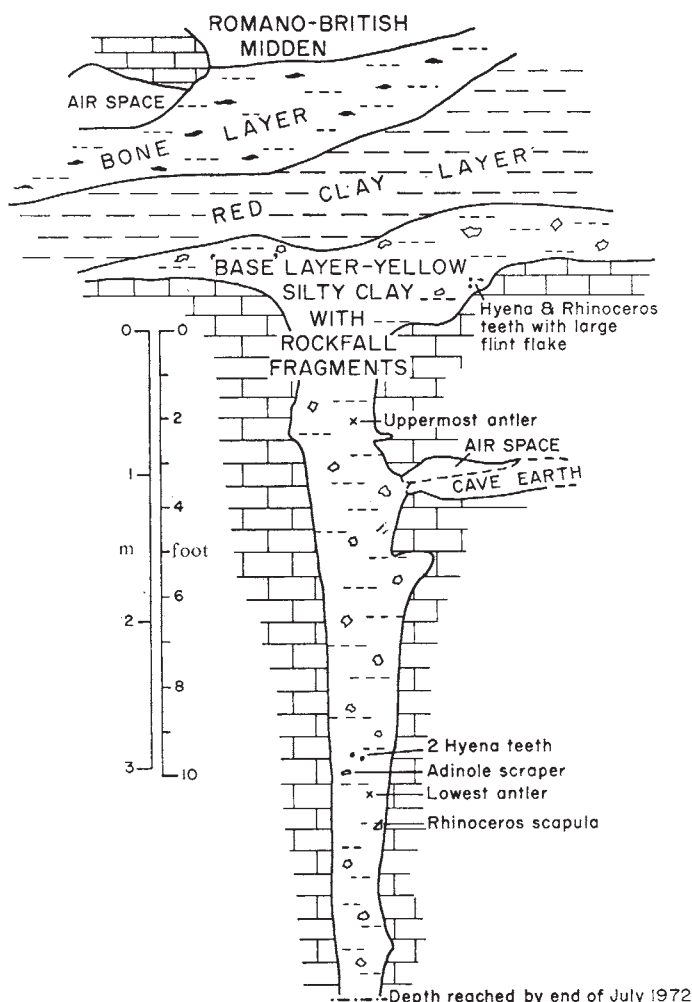


Fig. 1 Ogof-yr-Ychen, Caldey Island. Section of chamber 4 entrance and pit.

Until 1969 these excavations had concentrated on seven caves or rock shelters and two open sites; but since 1970 detailed investigations by two of us (Br James and M. D.) in conjunction with other workers on Caldey Island and on the limestone coasts of the Castlemartin Peninsula have revealed several potentially important Upper Palaeolithic cave sites. Of these, the most intensively studied so far is Ogof-yr-Ychen (British grid ref.: SS147968), an almost completely sealed cave on a vertical Carboniferous Limestone cliff at Jones' Bay on the north-east coast of Caldey Island. Here we summarize the preliminary findings from one of the main chambers in the cave and report the first radiocarbon age determination for its Pleistocene mammalian fauna.

Ogof-yr-Ychen consists of six chambers in a complex short system extending from 27 m OD to about 7 m OD. Before excavation these chambers were all partially filled with cave sediments. The mouth of a deep pit was revealed on a ledge outside the entrance of what has become known as chamber 4. The stratigraphy of both the chamber and pit is shown in Fig. 1. The pit has a roughly rectangular mouth measuring 1.8 m by 90 cm, and near-vertical sides. It has now been excavated to a depth of 6.4 m, excluding the 2.1 m of strata above its mouth; further excavation is difficult because of lack of space.

The yellow silty layer, with clay and rockfall fragments, occupies the full excavated depth of the pit. It has no traces of stratification, and throughout it consists largely of angular limestone rubble and broken pieces of stalagmite and stalactite in a finer matrix. Between 3 and 3.6 m below the mouth of the pit were found a rhinoceros scapula, an

adinole scraper of Creswellian type, two hyena molars and one flake of adinole and another of chert. For approximately 2.4 m above this group of finds there are scattered fragments (thirty-five finds) of red deer antler in an apparently identical matrix. Near the top of the deposit hyena and rhinoceros teeth occur in association with a large flint flake, and above this the layer gradually merges into the overlying red clay layer which contains a smaller proportion of angular limestone fragments.

Above this the bone layer (which is not so well represented elsewhere in Ogof-yr-Ychen) yielded a series of Mesolithic microliths of patinated flint, in addition to many of the bones identified in the faunal list in the following communication. The Romano-British midden was identified by its content of several types of potsherds similar to those found at nearby Nanna's Cave⁹, and capping the whole sequence of deposits was a yellow blown sand and then modern quarry debris.

The simplest interpretation of this sequence of deposits, together with its content of faunal remains and artefacts, is as follows. The lowest yellow layer is in part a cold-climate rockfall deposit dating from the Weichselian stage. It is succeeded by a late-glacial or early postglacial colluvial horizon and a postglacial bone layer which gives way to the Romano-British midden and then the modern blown sand. This sequence can be correlated with the succession of Late Pleistocene deposits elsewhere on Caldey Island¹¹ and on various cliff-top sites of the south Pembrokeshire coast^{9,12}.

In order to test this interpretation of events the scapula of *Coelodonta antiquitatis* from the pit (Fig. 1) was submitted to the Birmingham University Radiocarbon Dating Laboratory for radiocarbon dating. The age determination, on the collagen fraction only, was reported as $22,350 \pm 620$ yr BP (Birm340). This was unexpectedly old, but confirms that the yellow silty base layer is of Weichselian age and that Late Pleistocene mammals were present on the South Wales coast during the waxing phase of the last glaciation. It is not known how much time elapsed between the death of the animal and the deposition of the bone in the pit. The adinole artefact only 30 cm above the bone apparently indicates the presence of a still later Creswellian human culture at Ogof-yr-Ychen; it is unlikely to be separated from the deposition of the rhinoceros scapula by a wide interval of time, and it may demonstrate the arrival of the Creswellian culture in South Wales at a time of extreme periglacial conditions well before 10,000 yr BP⁸.

Apart from the age determinations for the Ogof-yr-Ychen rhinoceros and the "Red Lady" of Paviland⁶, there is one other reported radiocarbon date from a Welsh cave. This is a date of $18,000 \pm 1,400/1,200$ yr BP (Birm146) obtained by Rowlands¹³ for a mammoth carpal from Ffynnon Beuno, Flintshire. Although this cave is a Proto-Solutrean site the archaeological importance of the radiocarbon date is limited since the precise situation of the bone is not known; it came from a collection held locally since 1885 (ref. 14). The cave is, however, sealed by glacial and fluvioglacial deposits laid down by the last Irish Sea glacier. If the date is reliable it demonstrates that the Irish Sea glacier did not reach the North Wales coast until after 18,000 yr BP. It follows that it must have been even later in reaching South Wales and the weight of evidence now suggests that it may not have crossed the South Pembrokeshire coast at all.

There are clearly many problems associated with the work at Ogof-yr-Ychen. Some of these may be resolved by the analysis and radiocarbon dating of human remains found in chamber 3, which promises to yield even more exciting results than those from chamber 4 reported here. Chamber 3 contains a wolf skull in a stratigraphic position closely similar to that of the rhinoceros scapula in chamber 4, but of greater importance are the remains of three individual humans (including skull fragments) from another "bone layer". These human remains are being analysed at the University of London Institute of Archaeology, and further

work is being carried out at Durham University on the pollen content and sedimentological characteristics of the strata. The data from chamber 3 may form the subject of another brief report on the completion of further radiocarbon age determinations. The archaeological evidence from Ogof-yr-Ychen will be analysed in detail by Professor W. F. Grimes.

BR JAMES VAN NEDERVELDE

*The Abbey,
Caldey Island,
Off Tenby, Pembrokeshire*

M. DAVIES

*The Nature Conservancy,
Bangor, North Wales*

B. S. JOHN

*Department of Geography,
Science Laboratories,
University of Durham*

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Faunal Remains from Ogof-yr-Ychen, Caldey Island

THE non-human osteological remains excavated at Ogof-yr-Ychen have provided further evidence of the Pleistocene fauna associated with cave deposits along the southern Welsh coastline. The list (Table 1) confirms many of the species found already from the general area¹ and introduces two examples of a species not previously recorded, the common vole *Microtus arvalis* Pallas.

Altogether more than three thousand items were excavated and, although many are fragments which defy accurate identification, there are good examples which suggest the existence of several individuals among some of the larger species, notably ox, bear, red deer and hyena.

Few of the bones suggest fashioning by man, but several show surface scratching associated with gnawing by hyenas. Some of the bones had undergone mineralisation but this was not a constant feature, and from the red clay layer and the yellow silty clay specimens were often soft and extremely friable. Many instances of damage could be attributed to over-zealous digging by excavators.

From the excavations, in addition to the species now listed, there were bones from brown hare (*Lepus europaeus* Pallas), rabbit (*Oryctolagus cuniculus* Linné) and brown rat (*Rattus norvegicus* Berkenhout), but the site association of the first two suggests that the brown hare was a recent introduction, even though elsewhere it can be traced back to the late

Table 1 Faunal List

Insectivora:

Hedgehog (*Erinaceus europaeus* Linné)
Common mole (*Talpa europaea* Linné)

Carnivora:

Spotted (Cave) hyena (*Crocota crocuta* Erxleben)
Wildcat (*Felis sylvestris* Schreber)
Lion (Cave) (*Felis leo* Linné)
Glutton (*Gulo gulo* Linné)
Badger (*Meles meles* Linné)
Wolf (*Canis lupus* Linné)
Fox (*Vulpes vulpes* Linné)
Cave bear (*Ursus spelaeus* Rosenmüller and Heinroth)
Brown bear (*Ursus arctos* Linné)

Perrissodactyla:

Woolly rhinoceros (*Coelodonta antiquitatis* Blumenbach)

Artiodactyla:

Wild boar (*Sus scrofa* Linné)
Red deer (*Cervus elephas* Linné)
Roe deer (*Capreolus capreolus* Linné)
Steppe wisent (*Bison priscus* Bojanus)
Auroch (Giant ox) (*Bos primigenius* Bojanus)

Rodentia:

Common vole (*Microtus arvalis* Pallas)

Compiled from finds in all chambers at Ogof-yr-Ychen.

Pleistocene in Britain. The brown rat is not known in British deposits of preglacial origin.

There is a large amount of sheep/goat and domestic ox material and this came principally from the Romano-British and other disturbed layers. A certain amount of shell material was excavated, but again this came from disturbed layers and represented marine and land forms conspecific with extant forms. Some fish bones are present among material from disturbed layers and, although unidentifiable, they probably indicate items of diet from former human occupants of the cave.

J. A. BATEMAN

National Museum of Wales,
Cardiff

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Trade Wind and Temperature Correlations down a Deep-sea Core off the Saharan Coast

IN discussions relating to any possible climatic change that may occur even within our own lifetime, it is apparent¹ that studies of Pleistocene climatic changes are becoming increasingly valuable. Obviously it is desirable to have as complete a picture as possible of the Pleistocene period, and towards this end we now report a method that seemingly gives an estimate of wind strength throughout the last 700,000 yr.

Arrhenius² attempted the first description of trade-wind variations. He argued that the layers of calcium carbonate enrichment in deep-sea cores from the equatorial Pacific could be attributed to increased wind strengths, and he found that these occurred during the glacial stages. The reasoning used is somewhat complex. Although increased wind stress leads to more upwelling, higher productivity and hence higher

deposition rate of biogenic carbonate, there is a concomitant increase in the rate of dissolution on the sea bottom. More recently, Hays and Peruzza³, working with two cores close to the Saharan coast just west of Dakar, conclude that the wind was generally stronger during glacials. Because of the close proximity of the coast, wind blown terrigenous material swamps the carbonate. It is argued that the large, erratic and rapid fluctuations observed in the carbonate percentage are more likely to be due to moderate changes in the input of the terrigenous component rather than large changes in the carbonate production. With this persuasive reasoning, it would seem that these specially placed cores are indeed sensitive indicators of wind strength variations.

Briefly we now offer measurements, on a deep-sea core, that ought to lead directly to quantitative estimates of wind strength. (A complete account of the mathematical treatment involved in a winnowing theory and the experimental details of the size distribution analysis will be presented elsewhere.) It turns out that there is some uncertainty about whether it is the speed or some function of the speed that is being recorded; and so, at this stage, we prefer to introduce a new term called the "vigour" of the wind system—this does have a precise mathematical definition.

Oxygen isotope analyses of foraminifera in the same core permit us to place the wind determinations within the established stratigraphic framework and confirm the relationship between episodes of high trade-wind vigour and northern hemisphere glaciation. That the record is essentially complete is confirmed by the presence of the Brunhes-Matuyama (0.7 m.y.) magnetic reversal near the bottom of the core. The core (V23-100, 21° 18' N, 22° 41' W, at a depth of 4,579 m) was obtained just north of the Cape Verde Islands by the RV Vema; 2 cm long portions every 10 cm down the core were kindly supplied to us by the Lamont-Doherty Geological Observatory.

In principle, the method is quite simple. Dust from the Sahara is carried out to sea in vast quantities⁴ and particles larger than about 7 µm, which are mainly quartz, preferentially settle out by gravitation rather than by scavenging, that is, rain washout or dry-air turbulent fallout. Gravitational settling over the sea modifies the initial equilibrium size distribution existing over the desert. According to Jaenicke and Junge⁵ this distribution, true for any kind of soil and for a wide particle size range, is

$$dN = \text{constant } dD/D^4$$

where dN is the number of particles in unit volume of air in the diameter range D to $D + dD$, with the constant depending on wind and soil conditions. This distribution is assumed to hold true right up to the coast. Hot air Saharan outbreaks are the principal dust transporting mechanism.

Observations by Carlson and Prospero⁶ show that most of the dust is carried in a high level duct of thickness $z \sim 3$ km. This Saharan air rises at the coast above the layer of cooler maritime air which is about 1.5 km thick. Vigorous anti-cyclonic winds spread the dust into a broad front which travels west at a drift speed $u \sim 8 \text{ m s}^{-1}$. Assuming no lateral spreading of the front and uniform concentration of dust within the duct it can be shown that the mass sedimentation rate of the gravitationally settling particles at the core site, distance $x \sim 800$ km from the coast, becomes

$$ds = \text{constant } (dD/D) k D^2 \exp(-k D^2 x / uz)$$

where k ($0.77 \times 10^6 \text{ cm}^{-1} \text{ s}^{-1}$) is the factor in Stokes's law for quartz falling in air. Assuming further that, for a particular climatic episode, (uz) is confined to fairly narrow limits about a mean $\langle uz \rangle$, then it is possible to express the cumulative weight percentage greater than or equal to particle diameter D as

$$100 \exp [(-kx/\langle uz \rangle)(D^2 - D_0^2)]$$

where D_0 ($\sim 7 \mu\text{m}$) is the critical size below which gravitational settling ceases to be important. It follows that a plot of $\log_{10}$ of the cumulative weight percentage against D^2 should yield a straight line for the large particles and that the slope should be $0.43 kx/\langle uz \rangle$.

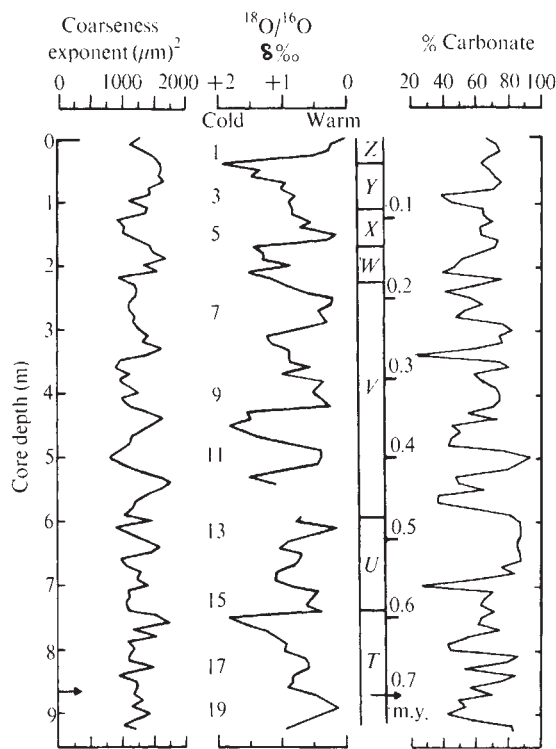


Fig. 1 The coarseness exponent compared with $^{18}\text{O}/^{16}\text{O}$ deviations from present day values and the total carbonate percentage, for core V23-100. The Z, Y, X... column is the *Globorotalia menardii* zonation¹⁰. The 1, 3, 5... column numbers only the warm periods in the climatic stages⁷⁻⁹. The ages, in m.y., are linearly interpolated between the top of the core and the magnetic reversal at the bottom.

We prepared a coarse concentrate ($\sim 0.4 \text{ g}$) from 1 to 2 g samples of the bulk carbonate-free core portions and the size distribution was measured by settling in a density-gradient sedimentation column. In every case down the core very good straight lines (not given here) were obtained, except for the largest particle size ($\sim 38 \mu\text{m}$) where probably insufficient quantities were recovered for accurate weighing. The reciprocal slope $\langle uz \rangle/0.43 kx$, a term which is called the coarseness exponent of the distribution, is plotted against depth down the core in Fig. 1. Assuming that x is constant then the exponent is directly proportional to $\langle uz \rangle$, the so called "vigour" of the wind system at that time. If z was constant in time, then the exponent would measure the drift speed of the dust fronts, but we cannot be sure that this is so. Inserting numerical values into the formula, a very good agreement with the observed coarseness exponent is obtained.

The oxygen isotope analytical techniques have been described elsewhere⁷. In the upper part of the core the planktonic species *Globorotalia menardii* was analysed. Stages (1) to (8) were easily correlated with the standard sequences^{8,9} as is the *Globorotalia menardii* zonation¹⁰. Unfortunately intense solution has affected certain sections of the core, which is not surprising considering the depth (4,579 m), and we were unable to recover sufficient specimens (about eight are needed) of *G. menardii* for analysis at a number of horizons. Because it is now known that the ^{18}O record is primarily due to the world-wide changes in ocean ^{18}O content (caused by the

accumulation of isotopically light ice on the continents during each glacial⁷), the more solution-resistant benthonic foraminifera may, however, be used with equal efficacy to derive a record. So as to display all analyses together, both planktonic and benthonic analyses are expressed in Fig. 1 as deviations (‰) from their respective present day values. This procedure obscures some of the fine details of the record but enables us to obtain a reliable stratigraphic framework despite the intense solution. The section comprising stages (9) to (11) is based almost entirely on analyses of benthonic foraminifera. A correction was made for isotopic disequilibrium in *Planulina wuellerstorfi*⁷. The other species used deposit carbonate at or near isotopic equilibrium.

In Fig. 1, ages are given on the basis of a linear interpolation between the top of the core and the magnetic reversal which was located at $866 \pm 5 \text{ cm}$ by N. D. Opdyke (Lamont-Doherty Geological Observatory). Comparison with the ages obtained by Shackleton and Opdyke⁷ for the stage boundaries in this time interval shows surprisingly good agreement considering the significant down-core variations in the sediment; they are probably more reliable than the somewhat younger ages that were current before the magnetic evidence became available^{8,9}.

From the rather good antiphase correlation between the coarseness exponent and the isotope deviations, it does seem certain that we can conclude, along with the other investigators, that strong trade winds are a general feature of the glacial stage—probably because of an enhanced temperature gradient between equator and poles. In our core the percentage of carbonate is quite high and shows no correlation with coarseness exponent. Presumably, changes in carbonate accumulation rate have more effect on the percentage than do changes in the terrigenous component, and so the Hays-Peruzza approach could not be used in this case. Even with cores that have low carbonate percentage, it is not absolutely certain that variations in percentage reflect variations in wind strength. This is because the amount of wind blown dust will depend very markedly on the soil conditions existing at the time; strong winds will not create dust storms over damp vegetation-covered soil. Hays and Peruzza have, however, strengthened their argument by noting a higher percentage of silt-sized particles in the clay deposited during episodes of low carbonate.

Finally, it is interesting to note from Fig. 1 that the limits within which the wind vigour varies have remained fairly constant—just as the limits within which the northern hemispheric ice volume, and sea temperatures, are believed to have done. This is quite remarkable, and any theory of ice age causation must take these facts into account.

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D. W. PARKIN

School of Physics,
University of Bath, Bath

N. J. SHACKLETON

Sub-department for Quaternary Research,
University of Cambridge,
Cambridge

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Free Energy of Non-isothermal Systems

HERE I draw attention to two thermodynamic functions the validity of which seems to be beyond dispute but the very existence of which is little known. Their importance is almost completely disregarded even though they extend the application of the free-energy concept to all systems, isothermal or not: the familiar free-energy functions of Helmholtz and Gibbs, A and G respectively, are simply special cases of the two more universal functions.

First, I consider whether the maximum work is a thermodynamic function of state. That is, when a system in a particular environment passes from one defined state to another, is the maximum work that could be obtained, $w_{\max}$, always a fixed quantity independent of the path followed? It is well known to be so under isothermal conditions for a closed system at a uniform constant internal temperature T_s which is the same as that of the environment T_e ; $w_{\max}$ is then given by $-\Delta A$ (or $-\Delta G$ if pressure-volume work is to be excluded); and both A and G are functions of state.

It seems not be widely known that a similar statement is valid even when the restriction to isothermal processes is lifted. For example, Butler writes (ref. 1, page 62) concerning changes of internal energy U : "But of the possible ways of conducting the change, there will usually be one for which w is a maximum and, therefore, q also a maximum". His use of the word "usually" implies that he believed there to be exceptions, but of these no further details are given. Later (ref. 2, page 257), he does state: "The maximum work of a change is, as we have seen, a definite quantity only so long as the temperature remains constant throughout" so this is presumably the restriction implied in his earlier quotation. This is puzzling because it conflicts with his statement of the Second Law (ref. 2, page 32) which is not, apparently, intended to apply solely to isothermal conditions but which nevertheless is founded on the idea that $w_{\max}$ always has a definite value. Keenan^{3,4} and Denbigh⁵ leave no doubt that $w_{\max}$ is a function of state even for non-isothermal conditions. But Denbigh's section is in small print and could easily be overlooked: many authors ignore the question entirely.

The history of this subject is hard to disentangle. A statement of the simplest free-energy function was given in an engineering context by Darrieus⁶, who states that "the topic was considered successively by Maxwell, Lord Kelvin and Gibbs", but does not give detailed references. A general derivation was published independently by Milne (ref. 2, page 231) who reworked and extended the proofs of the familiar free-energy equations and came to the conclusion—not to be found in the particular passages by Gibbs on which he was commenting—that it was possible to extend "the physical significance of the work function" (the Helmholtz function) "to processes of non-constant temperature". He considered a system that started at temperature T_s , underwent some change (which could, for example, be a chemical reaction), and finally came into thermal equilibrium with a large reservoir at the constant lower temperature T_e . He concluded that the maximum work obtainable from a system undergoing such a sequence of changes was given by the equation

$$\Delta W = -\Delta A + S_s(T_s - T_e) \quad (1)$$

where S_s is the entropy of the system at temperature T_s .

This is an awkward equation to use, or even to think about, because of the appearance in it of the absolute entropy of the system S_s , but at least one important conclusion can be drawn from it directly: since all the terms on the right hand side of the equation are functions of state, ΔW must be one also. It is better to substitute into equation (1) the general expression for the Helmholtz function ($A = U - TS$), rearranging to

$$\Delta W = \Delta U - T_e \Delta S \quad (2)$$

I have changed the sign of Milne's ΔW to conform with the universal practice for A and G , thus

$$w_{\max} = -\Delta W \quad (3)$$

Equation (2) is the most basic of all the free-energy equations, applicable equally to non-isothermal and to isothermal situations: in the latter case, with $T_e = T_s$, it reduces to the familiar Helmholtz equation. In spite of its importance, the function described by equation (2) does not have a name, so it could not be indexed even if authors chose to write about the subject. Following Milne one might name it "work function", but this has been pre-empted for the electronic work function. The name "work content" would be descriptive: it was at one time used for the Helmholtz function¹ so it might be thought apt for the more general form of that function.

A second non-isothermal free-energy function, the availability, B , had already been described by Keenan³. He did in fact discover this for himself as a maximum work function (personal communication), but soon found out that the same function had been used by Gibbs in discussing equilibria as early as 1873 (ref. 7). Gibbs called it "the available energy of the system and medium" which indicates that he was well aware of its use for determining maximum work. Put in the same form as equation (2)

$$\Delta B = \Delta U - T_e \Delta S + p_e \Delta V \quad (4)$$

where p_e is the environmental pressure and ΔV is the change in volume. $-\Delta B$ thus measures the capacity for performing work, excluding as irrelevant any work performed against the ambient pressure. There is no evidence that Milne⁷ knew of the work of Darrieus⁶ or of Keenan³; or for that matter, of the relevant work by Gibbs. There have probably been many other "discoveries" of the same facts and they will doubtless continue until these functions are better known.

The availability, B , is generally encountered in an applied context, sometimes with the implication that it is of restricted applicability. Thus Lee and Sears⁸ state that whereas the availability is a composite property of the system and its environment (correct), Gibbs's function is a property of the system alone. This is erroneous because the condition $T_e = T_s$ must be satisfied before Gibbs's function can be employed at all; the pressure-volume term likewise involves the ambient pressure p_e which may or may not be the same as that within the system. In fact B is less restricted than G , not the converse.

W and B have several uses.

(i) Calculation of maximum work in both isothermal and non-isothermal processes from equation (2) or (4). Which of the two is appropriate depends on the status of the pressure-volume work in the particular case under consideration, for it is by no means automatically "useless". In biological systems (which incidentally are almost universally isothermal) $p_0 \Delta V$ work is generally negligibly small; but in the cases where it is not it may be both large and important. Two examples are provided by the buoyancy organs of some marine animals⁹ and the generation of carbon dioxide by yeast, which can continue despite ambient pressure exceeding 1,000 atm (ref. 10).

Pressure-volume work differs from other kinds of work in that it is the only type that a system can perform without a "machine".

(ii) Equation (2) provides the most general statement for equilibrium and for thermodynamic reversibility applicable to a non-isolated system in an environment at constant temperature

$$dw = -dW \quad (5)$$

If the system is one that produces no external work other than pressure-volume work

$$dw = p_e dV \quad (6)$$

and the condition for equilibrium becomes

$$dB = 0 \quad (7)$$

(iii) Equation (2) provides the most general expression for efficiency ϵ of any process

$$\epsilon = w / -\Delta W \quad (8)$$

Note that W and not B should be used in this case. If the pressure-volume work is not harnessed it represents wasted work, which can only be recovered in certain restricted types of cyclic process.

(iv) Equation (2) provides a general formula for calculating the entropy created, ΔS_{cr} , during any specified spontaneous process.

$$\Delta S_{cr} = [(-\Delta W) - w] / T_e \quad (9)$$

The expression in square brackets may justly be called the "wasted work" (ref. 5, page 71), that is, the work that might have been obtained but was not.

Thus

$$\Delta S_{cr} = [\text{wasted work}] / T_e \quad (10)$$

Equation (9) is easily derived from equation (2) because

$$[(-\Delta W) - w] = -\Delta U + T_e \Delta S - w = -q + T_e \Delta S \quad (11)$$

since

$$w - q = -\Delta U \quad (12)$$

where $-q$ is the heat given by the system to its surroundings which are at T_e . Therefore

$$[(-\Delta W) - w] / T_e = -q / T_e + \Delta S \quad (13)$$

The first term on the right hand side is the increase in entropy of the surroundings and the second is the entropy change in the system, so their sum is the total entropy change in the Universe, that is the entropy created, ΔS_{cr} . In this context too, W , and not B , is the appropriate function to employ. In the special case of slow expansion against an infinite atmosphere the pressure-volume work is stored as mechanical energy and this does not result in the creation of entropy, so B could be used: however, this is not the case in general.

Equation (9) provides the basis for my claim¹¹ that a statement of the Second Law in terms of work production is equivalent to the more familiar statement in terms of entropy creation.

Keenan made extensive and detailed applications of the availability B in considering the inefficiency of heat engines and at what stage in their operation it arose⁴. The only chemical process that he considered was combustion, which he showed

to be the principal source of waste. (For this reason fuel cells are used nowadays when high efficiency is essential.)

Denbigh¹² extended this application to chemical processes in general, using the availability to establish criteria of performance for a chemical factory in which heat movements as well as yield of product were taken into account. Such ideas have become of increasing importance recently, partly because they have become realizable through new developments in engineering. For example, fuel cell technology can now be used both for power production and for performing a surprisingly wide range of chemical processes¹³; semiconductor thermoelements and efficient Stirling engines permit inter-conversion of heat and work in circumstances where this was previously impossible.

The recent realization that our supply of fossil fuel is limited is of pressing importance, emphasizing the need for us to make the most efficient possible use of it. Practical consideration is at last being given to the notion of total utilization whereby the fuel's potential for chemical synthesis is considered alongside its capacity for power production^{14,15}. To assess schemes of this sort the functions W and B are essential.

They seem to have been "discovered" on several occasions, and one final reason for wishing that they were better known is to spare others the labour—and the disappointment—of "discovering" them yet again (as I did last summer).

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D. R. WILKIE

Department of Physiology,
University College of London,
Gower Street, London WC1E 6BT

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BIOLOGICAL SCIENCES

Cyclic GMP and Cell Movement

THE movement of mammalian cells can be inhibited by agents which mimic or produce increases in the intracellular levels of cyclic 3',5'-adenosine monophosphate (cyclic AMP)^{1,2}. Goldberg and coworkers^{3,4} have proposed that in a number of cell types cyclic 3',5'-guanosine monophosphate (cyclic GMP) promotes cellular events that are antagonistic to those believed to be mediated by cyclic AMP. One method of testing this hypothesis would be to examine the effects on leukocyte chemotaxis of agents that should

stimulate an increase in cellular cyclic GMP concentration. Both phorbol myristate acetate (PMA)⁵ and acetylcholine (ACh)³ seem to produce such an effect on more than one cell type and these compounds were therefore used to investigate the possibility that cyclic GMP may play a role in modulating cell movement.

The experiments described here suggest that the leukotactic response of polymorphonuclear leukocytes is stimulated by agents such as PMA, acetylcholine and carbamylcholine (Cch). Imidazole, a compound known to produce cholinergic-like effects in a variety of tissues³, also enhanced leukotactic activity. The 8-bromo analogue of cyclic GMP also increased leukotaxis, which was inhibited in the same experimental conditions by dibutyryl cyclic AMP.

A modification of the procedure of Boyden^{6,7} was used to assess leukotactic activity. Leukocytes were preincubated in the agents indicated for 5–10 min at 37° C in tissue culture medium 199 (Microbiological Associates, Albany, California), after which they were washed and a known quantity of PMN deposited on a 'Millipore' filter with a Shandon cytocentrifuge. The filter was placed in a modified Boyden chamber and a chemotactic stimulus derived from a culture filtrate of *E. coli*⁸ was added to the attractant side of the chamber. After incubation for 3 h at 37° C, the number of cells that had migrated through the filter within ten random fields was determined and a leukotactic index calculated by dividing the number of PMN that had migrated through the filter by the total number of PMN (in hundred thousands) delivered to the starting side of the filter. All leukotactic indices are averages of three samples. Using this method the standard deviation is 10% of the mean. The standard error of the mean of samples tested on separate occasions is 6%.

Table 1 Leukotactic Responses of Control Neutrophils to PMA and Bacterial Chemotactic Factor

Attractant	Leukotactic index
199	12
1.0 ng PMA	28
10 ng PMA	30
BF	54
BF + 1 ng PMA	380
BF + 10 ng PMA	388

Table 2 Effect of Leukocyte Preincubation with PMA on Leukotaxis

Preincubation	Leukotactic index
199	31
0.01 ng PMA	63
0.1 ng PMA	286
1.0 ng PMA	300

PMA had only minimal inherent chemotactic activity for neutrophils when added alone to the attractant side of the Boyden chamber (Table 1). However, when *E. coli* bacterial chemotactic factor (BF) was also added to the attractant side of the chamber 1–10 ng of PMA ml⁻¹ produced a significant enhancement (greater than seven-fold) of leukotactic activity. To eliminate the possibility that the enhanced leukotactic activity resulted from an interaction of PMA with BF, cells were preincubated with PMA (0.01 to 1 ng ml⁻¹) for 5–10 min, washed and then tested for responsiveness to the BF in media without added PMA. This pulse exposure of the leukocytes to PMA also resulted in a marked enhancement (up to five-fold)

in leukotactic activity (Table 2). Because PMA may have effects on cell function other than those on cyclic GMP levels, acetylcholine and carbamylcholine were also tested and found to produce effects similar to those observed with PMA (Table 3). Moreover, the ability of these cholinergic agents to stimulate leukotaxis was inhibited by atropine.

Imidazole, a compound which exhibits cholinergic-like activity in a number of biological systems and also been shown to be inhibitory to cyclic nucleotide phosphodiesterase promoted hydrolysis of cyclic GMP¹⁰, also produced an increase in leukotactic activity (Table 4). Preincubation of leukocytes with imidazole and PMA produced an approximately additive effect.

Table 3 Effect of Leukocyte Preincubation with Cholinergic Agents on Leukotaxis

Preincubation	Leukotactic index
199	59
Ach 10 ⁻⁵ M	154
Ach 10 ⁻⁶ M	94
Ach 10 ⁻⁶ M + Atr 10 ⁻⁷ M	52
199	46
Cch 10 ⁻⁵ M	153
Cch 10 ⁻⁵ M + Atr 10 ⁻⁶ M	70

Table 4 Effect of Leukocyte Preincubation with Imidazole and PMA on Leukotaxis

Preincubation	Leukotactic index
TC 199	40
1.25 mM imidazole	66
0.005 ng PMA	89
1.25 mM imidazole + 0.005 ng PMA	136

Dibutyryl cyclic AMP and 8-bromo cyclic GMP are analogues of the parent cyclophosphates which are believed to cross cell membranes more easily and/or undergo less rapid enzymatic degradation than their naturally occurring cyclic nucleotide counterparts. Preincubation of leukocytes with 10⁻² to 10⁻⁴ M dibutyryl cyclic AMP depressed leukotactic responsiveness, while preincubation with 8-bromo cyclic GMP enhanced the response to chemotactic stimulation (Table 5). The effect of 8-bromo cyclic GMP was not blocked by atropine, however.

Table 5 Leukotactic Activity of Neutrophils following Preincubation with Cyclic Nucleotides

Preincubation	Leukotactic index
TC 199	46
8-Br Cyclic GMP 10 ⁻⁵ M	134
8-Br Cyclic GMP 10 ⁻⁵ M + Atr 10 ⁻⁵ M	131
TC 199	41
db Cyclic AMP 10 ⁻² M	26
db Cyclic AMP 10 ⁻³ M	18
db Cyclic AMP 10 ⁻⁴ M	15

Cholinergic agents as well as cyclic GMP (and derivatives) have recently been shown to enhance degranulation of PMN^{9,11}, whereas cyclic AMP inhibits both leukotaxis² and degranulation¹². These findings are consistent with the Yin Yang hypothesis that the two cyclic nucleotides have opposing actions in bidirectionally controlled systems.

During exposure of cells to dibutyryl cyclic AMP, the leukotactic index corresponds to that found in the absence of chemotactic attractant. This suggests that only leukotactic movement, and not random or basal movement, may be affected. In this case, the potentiation of leukotaxis by PMA or other agents may derive from their ability to potentiate an increase in cyclic GMP levels already induced by the chemotactic factor. Other agents which act on the membrane and enhance chemotaxis such as cytochalasin B¹³ may act by mechanisms similar to that proposed for the agents used in these experiments. The effect of 8-bromo cyclic GMP may be due to its presumed intracellular action or to direct effects on PMN plasma membrane.

Finally, modulation of cell movement by cyclic nucleotides may be an important factor in both malignancy and embryonic development. The property of invasiveness characteristic of malignant cells might be enhanced as a result of increased cyclic GMP concentration in these cells. In embryogenesis hormones or cell-cell surface contacts might be providing the stimulus for increases in cellular cyclic GMP or cyclic AMP that modulate cell movement in a developing embryo and the levels of these cyclic nucleotides could determine or modulate initiation or termination of movement of cells. Cyclic GMP might provide a "go" and cyclic AMP a "stop" signal. In this case the "stop" signal may also be a signal for a differentiation as has been suggested by experiments with neuroblastoma cells in culture¹⁴.

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R. D. ESTENSEN

Department of Laboratory Medicine and Pathology

H. R. HILL
P. G. QUIE
N. HOGAN

Department of Pediatrics

N. D. GOLDBERG

Department of Pharmacology,
University of Minnesota,
Minneapolis, Minnesota 55455

Received July 20, 1973.

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Uniformity of Radiation-induced Mutation Rates among Different Species

ONE of the major difficulties in estimating the genetic hazards of ionising radiation to human populations has been our inability to extrapolate with confidence from mutation rate data in lower organisms to man^{1,2}. Experimentally observed mutation rates per locus per rad extend over an enormous range of three orders of magnitude. For instance, the forward mutation rate per locus per rad is 1×10^{-9} in *Escherichia coli* B/r at two loci for phage T1 resistance, 2.7×10^{-9} in *Neurospora* at the *ad3* locus, 1.4×10^{-8} in *Drosophila* for eight specific loci (averaged), 1.7×10^{-7} in the mouse for twelve specific loci (averaged), and 1×10^{-6} in barley for three loci.

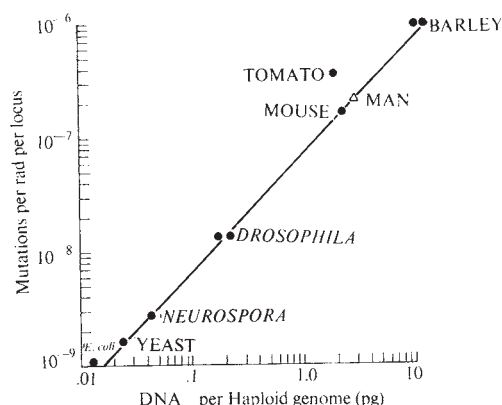


Fig. 1 Relation between forward mutation rate per locus per rad and the DNA content per haploid genome. Line drawn with slope of one through the mouse point. Point for man estimated from DNA content.

The development of target theory in radiobiology and the recognition that the genetic material is the target for many radiobiological endpoints led to the demonstration that the mean lethal dose for survival of irradiated single cells is related to the nuclear DNA content^{3,4}. Sparrow *et al.*⁵ have noted that in plants, radiosensitivity is closely related to several nuclear parameters such as nuclear volume and interphase chromosome volume and thus to the DNA content per nucleus. In amphibians, too, it has been shown that the mean lethal dose for survival of whole-body irradiated animals is directly related to the amount of DNA per nucleus and per chromosome⁶. The work of Sparrow and his coworkers⁷⁻⁹ has demonstrated that the relation between nuclear parameters and radiosensitivity is not entirely simple, however, for although the organisms

studied do fall into a series of 'radiotaxa' within which a simple linear relation holds, the sensitivities of the radiotaxa differ from one another.

We re-examined the data for radiation-induced forward mutation rates from organisms as disparate as bacteria, fungi, higher plants, insects, and mammals for which we also know the amounts of DNA per nucleus. As seen in Table 1, and as noted above, the mutation rates per locus per rad vary over three orders of magnitude. Nonetheless, when these rates are adjusted for the amount of DNA per nucleus and thus normalised to a common biological baseline, the mutation rates obtained are all essentially the same, varying by a mere factor of three instead of 1,000.

of the functional genetic unit would fail to complement mutations in any of the other components¹⁴.

Models can also be postulated in which the repair processes vary quantitatively in the different species.

Whatever the basic explanation might be, we conclude that, empirically, the radiation-induced mutation rate is proportional to the total genome size (DNA content) for the species. This correspondence, which is shown graphically in Fig. 1, allows us to extrapolate from mutation rates obtained in experimental organisms to man with greater confidence.

This work was performed under the auspices of the US Atomic Energy Commission and supported in part by a

Table 1 Specific Locus Mutation Rates Normalized for Amount of DNA per Nucleus

Organism	DNA per haploid genome (pg)	Relative amount of DNA as compared with human A	Specific locus mutations per locus per rad B	Normalised specific locus mutations per locus per rad B/A	References
<i>E. coli</i> B/r (Bacterium)	0.013 *	4.5×10^{-3}	$1 \times 10^{-9} \dagger$	2.2×10^{-7}	*15, †16
<i>Saccharomyces cerevisiae</i> (Yeast)	0.024 *	8.3×10^{-3}	$1.6 \times 10^{-9} \dagger$	1.9×10^{-7}	*17 †R. Mortimer, personal communication
<i>Neurospora crassa</i> (Fungus)	0.042 *	1.4×10^{-2}	$2.7 \times 10^{-9} \dagger$	1.9×10^{-7}	*17, †18
<i>Drosophila melanogaster</i> (Fruit fly)	0.17 * 0.22	0.058 0.076	$1.4 \times 10^{-8} \dagger$ (Spermatogonia)	2.4×10^{-7} 1.8×10^{-7}	*19 †20, 21
<i>Lycopersicon esculentum</i> (Tomato)	1.95 *	0.67	$3.75 \times 10^{-7} \dagger$	5.6×10^{-7}	*22 †23
<i>Mus musculus</i> (Mouse)	2.26 *	0.78	$1.7 \times 10^{-7} *$ (Spermatogonia)	2.2×10^{-7}	*2
<i>Hordeum vulgare</i> (Barley)	10 * 11.5 †	3.45 3.97	$1 \times 10^{-6} \dagger$	2.9×10^{-7} 2.5×10^{-7}	*24, †17, †25
<i>Homo sapiens</i> (Man)	2.9	1	—	2.6×10^{-7} (unweighted average)	

This remarkable consistency in normalised mutation rates in such a wide variety of organisms strongly suggests that extrapolation directly from experimental organisms to man can be done with confidence. For all the species listed in the table, the unweighted average of the mutation rates normalised to the human DNA value is 2.6×10^{-7} per locus per rad. (These rates are all for forward mutations induced by acute irradiation. With chronic radiation or with germ cells irradiated during different cell stages, the mutation rates often vary by a factor of three to four. These differences are conventionally attributed to differential repair. Back mutations, which are often the result of base changes in suppressor loci, are usually lower by a factor of ten.)

The consistency that obtains when the data are adjusted for the amount of DNA per nucleus for each species indicates either that it is the nucleus, and not the locus, that determines target size, or that, on the average, the size of a (radiation-mutable) locus is proportional to the total genome size (DNA content) for the species.

Although the latter conclusion is contrary to the usual genetic expectation that structural genes should be the same size in bacteria as in man, we note that the functional genetic unit in *Drosophila* corresponds to a band in the salivary gland polytene chromosomes¹⁰. This indicates that complementation groups in higher forms contain much more DNA than is necessary for specification of a protein. It has been postulated that the extra DNA has a regulatory function¹¹⁻¹³ and that mutations in any of the components

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SEYMOUR ABRAHAMSON

Departments of Zoology and Genetics,
University of Wisconsin,
Madison, Wisconsin 43713

MICHAEL A. BENDER

Department of Radiology,
The Johns Hopkins University School of Medicine,
Baltimore, Maryland 21205

ALAN D. CONGER

Department of Radiobiology,
Temple University School of Medicine,
Philadelphia, Pennsylvania 19140

SHELDON WOLFF

Laboratory of Radiobiology and Department of Anatomy,
University of California,
San Francisco, California 94143

Received August 30, 1973.

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Sub-lethal Effect of Sulphaquinoxaline on the Tsetse Fly, *Glossina austeni* Newst

A MARKED fall in fecundity of females of the tsetse fly, *Glossina austeni* Newst, fed on rabbits, was found to be associated with the use of a commercial rabbit diet containing, as a coccidiostat, 75 p.p.m. sulphaquinoxaline and 7.5 p.p.m. pyrimethamine. Contemporary flies fed on goats reproduced normally.

Table 1 Performance of Female *G. austeni* fed on Rabbits Supplied Successively with Various Diets.

	No. ♀♀	% ♀♀ surviving to day 60	No. puparia produced	Mean No. puparia produced per ♀
Diet A	150	86.7	1,362	9.1
Diet B	243	81.5	624	2.6
Diet C	150	72.0	1,295	8.6

Diets: A, Coccidiostat-free; B, 75 p.p.m. sulphaquinoxaline and 7.5 p.p.m. pyrimethamine; C, coccidiostat-free.

The performance of female flies fed on rabbits given the coccidiostat diet (diet B) is compared in Table 1 with the performance of female flies fed on the same rabbits given coccidiostat-free diets before (diet A) and after (diet C) the 3 month period of feeding the treated diet. Although the longevity of all groups of flies was similar, females fed on rabbits

supplied with diet B produced markedly fewer puparia than those fed on the rabbits when supplied with diets A and C. Age-specific fecundity schedules¹ show that the production of puparia by the affected flies was only slightly sub-normal during the first reproductive cycle but that in succeeding reproductive cycles a number of aborted larvae, interspersed with occasional normal larvae, were produced, followed by most females ceasing reproduction.

The observed effect on the fecundity of *G. austeni* is considered to have resulted from the action of sulphaquinoxaline. This, and other sulphonamide compounds, have systemic insecticidal activity against mosquitoes after intraperitoneal administration to the host animal^{2,3}. Single doses of 500 to 540 mg kg⁻¹ of sulphaquinoxaline were required for a 48 h LD₅₀ value. Interference with the synthesis of folate, directly or on a gut symbiont, has been suggested as the mode of action of sulphaquinoxaline under such circumstances⁴. Pyrimethamine alone is inactive as a systemic insecticide⁵.

The effects on haematophagous arthropods resulting from feeding their hosts with concentrations of sulphaquinoxaline as low as 75 p.p.m. do not seem to have been reported, although in one case 330 p.p.m. effected control of northern fowl mite, presumably by killing the mites⁶. In *G. austeni*, 75 p.p.m. was insufficient to cause any increase in mortality but did result in marked interference with the ability of females to produce offspring. The relatively complicated method of reproduction of *Glossina*, adenotrophic viviparity, may be particularly susceptible to interference by very small quantities of such compounds as sulphaquinoxaline but workers maintaining any species of haematophagous arthropod should perhaps guard against the use of coccidiostats in the diet of their host animals.

In exceptional conditions in Africa, *Glossina* spp. have been reported as being virtually dependent on one species of domestic animal, generally cattle^{6,7}, for food. In such circumstances, regular treatment of the hosts with low and acceptable doses of sulphaquinoxaline might have potential for the localized control of *Glossina*.

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A. M. JORDAN
M. A. TREWERN

*Tsetse Research Laboratory,
University of Bristol,
School of Veterinary Science,
Langford, Bristol, BS18 7DU*

Received July 17, 1973.

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Role of Calcium in the Seasonal Modulation of Pacemaker Activity in a Molluscan Neurosecretory Cell

MOLLUSCAN ganglia contain several pacemaker neurones that spontaneously generate slow membrane potential oscillations associated with a burst of spikes during the depolarising phase (bursting pacemaker potential, or BPP)¹⁻⁵. One such neurone,

a neurosecretory cell in land snails, produces BPPs when the snail is active, but is typically silent and could not induce BPP activity during aestivation (hibernation)⁶⁻⁸. We report here that calcium is important in the seasonal modulation of this cell's BPP activity.

We used a neurosecretory cell (cell No. 11 according to Gainer's nomenclature⁶⁻⁸) in specimens of the land snail *Otala lactea*, which had either been in aestivation for more than 2 yr ('aestivated') or were fully activated, or were in a transitional state between aestivation and activity. Hibernated snails which had been maintained for 2 yr in a well-aerated wicker basket at 6° C were transferred to room temperature (20–23° C) 1 month before use and divided into two groups. The first group which was kept dry, and therefore remained in experimental diapause or 'aestivation', did not remove their calcareous epiphagms nor emerge from their shells during the holding period. The second dormant group was transformed into 'active' snails by maintenance in high humidity for 1 month. Periodically fed lettuce, these snails exhibited normal foraging and mating behaviour. 'Transitional' animals had been activated for 1–10 d.

In a typical experiment the ganglia were removed, pinned to Sylgard in a plastic Petri dish and perfused with snail saline, which consisted of (mM): 80 NaCl, 4 KCl, 5 MgCl₂, 10 CaCl₂ and 5 Tris-HCl buffered to pH 7.8. One or two micro-electrodes were placed in the identified neurosecretory cell and electrical activity was monitored by conventional techniques. Ion changes in the media were effected by washing the preparation with a twenty-fold excess volume of the new solution. To change the divalent cation concentration of the bathing solution the appropriate divalent (Ca²⁺, Mg²⁺ or Sr²⁺) as the chloride salt was added to a divalent-cation-free stock solution containing (mM) 100 NaCl, 4 KCl and 5 Tris-HCl buffered to pH 7.8. Hyperosmolarity of salines containing more than the normal divalent cation concentration was controlled for by adding NaCl instead of a divalent cation. NaCl did not produce the regulatory effects observed with divalent cations. The experiments were conducted at room temperature (20°–23° C).

Cells from aestivated snails were quiescent, having a greater resting membrane potential and conductance than cells from active animals, as previously reported⁶⁻⁸. The current-voltage relations of the aestivated cells were virtually linear over the –40 to –80 mV range while those of active cells were markedly alinear exhibiting both anomalous and delayed rectification⁶⁻⁸. Sustained depolarisation of the membrane potential of aestivated cells by intracellular current elicited repetitive firing of spikes without BPP activity (DEPOL, Fig. 1a). When calcium was removed from the saline, the membrane potential decreased and the cell began to generate BPPs which could be maintained as long as magnesium remained in the medium. The appearance of BPP activity was accompanied by a change from linear to markedly alinear current-voltage relations (not illustrated). Thus, the removal of calcium from the external medium transformed the electrical properties of the aestivated cell into those of an active cell.

Cells from 'transitional' snails exhibited spontaneous pacemaker activity without slow membrane potential oscillations (Fig. 1b). Current injection of either polarity could induce the BPP rhythm under normal saline conditions. Calcium-free saline did elicit BPP activity. If the remaining divalent cation (magnesium) was removed, the membrane potential gradually depolarised, lost its ability to generate the BPP and eventually reached a high conductance, low potential state characterised first by fast frequency spike activity and then by inactivity. In these conditions injection of hyperpolarising current could not restore the BPP, indicating that the presence of magnesium was necessary for the generation of BPPs.

Removal of calcium from the saline bathing 'active' cells spontaneously generating BPPs led to a depolarisation and

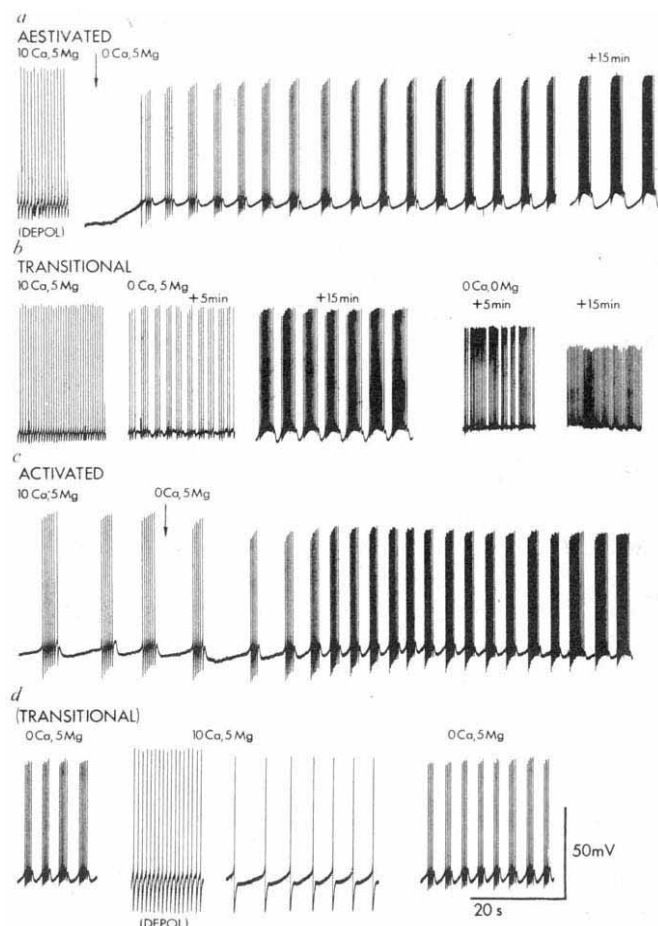


Fig. 1 Calcium regulates the bursting pacemaker potential (BPP) of a neurosecretory cell. Membrane potential records from the identified neurosecretory cell (No. 11) in specimens of the land snail *Otala lactea* at different states of activity. *a*, Example from an aestivated snail. In physiological salines ($[Ca^{2+}]_o = 10$ mM) the cell, which is hyperpolarised and silent, responds to sustained injection of depolarising current with repetitive activity (DEPOL). Removal of $[Ca^{2+}]_o$ from the medium leads to a depolarisation and initiation of spontaneous bursting activity, which progresses and stabilises with time. *b*, Example from a snail in transition from aestivation to activation. Although the cell is spontaneously firing under control conditions, it does not burst with application of either hyper- or depolarising trans-membrane current (not shown). Removal of $[Ca^{2+}]_o$ from the medium induces bursting behaviour. Removal of $[Mg^{2+}]_o$, resulting in a nominally divalent cation-free solution, leads to a depolarisation and high rate of firing of spikes with subsequent loss of the BPP. *c*, Example from an active snail. The bursting rhythm is present in control saline. The frequency of bursts, as well as spikes per burst, increases while the BPP amplitude decreases on removal of $[Ca^{2+}]_o$. *d*, A transitional cell bursting in calcium-free solution stops bursting and fires repetitively when the physiological complement of $[Ca^{2+}]_o$ is added to the medium (middle record) despite sustained depolarising current (DEPOL). Return to the calcium-free condition restores the burst.

increase in the frequency of bursts as well as spikes per burst (Fig. 1c). If magnesium was also removed, then the membrane potential gradually depolarised, the BPP disappeared and the cell lost its ability to generate BPPs in spite of hyperpolarising current. The latter condition was seen with cells obtained from animals in all three environmental states.

The physiological role of calcium in the regulation of BPP activity is further illustrated by Fig. 1d. A cell from a 'transitional' snail spontaneously generates BPPs in a calcium-free saline containing magnesium. Addition of the physiological complement of calcium (10 mM) to the saline completely inhibited BPP generation. Removal of the calcium then restored the BPP activity.

To investigate further the divalent cation regulation of the BPP we compared the BPP activity of these cells in the presence of varying concentrations (0–60 mM) of calcium, magnesium and strontium. These experiments were conducted by washing in divalent-cation free saline until the ability to generate BPP activity disappeared, after which the appropriate divalent cation was added to the saline and BPP activity monitored by measuring the amplitude of the BPP (defined in the inset of Fig. 2). Intracellular injection of current was always used to test the ability of the cells to generate BPP activity since the membrane potential was frequently depolarised in low divalent cation strength solutions and hyperpolarised in high calcium and magnesium solutions. Low strength divalent cation solutions were also associated with a decrease in threshold for firing as well as a decrease in input resistance, while the opposite effects were seen in high strength divalent cation solutions. Since all three divalent cations produced similar changes in these parameters, their differential effects on BPP activity were compared without taking these changes into account. The amplitude of the BPP was always measured at the current just necessary to produce the BPPs.

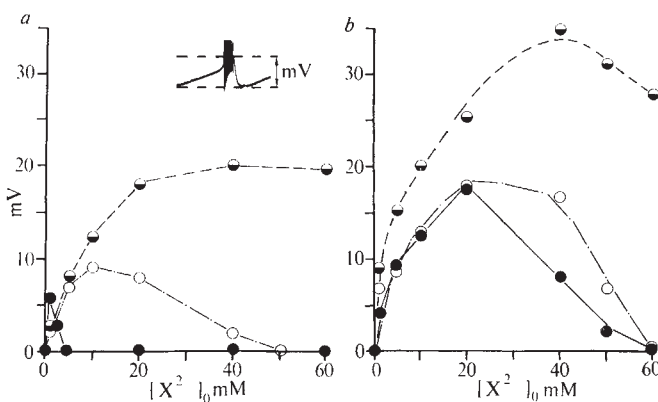


Fig. 2 Bursting pacemaker potential amplitude varies with external divalent cation concentration, type ($[X^{2+}]_0$), and state of activity of the snail. BPP amplitude (mV) is defined in the inset in (a) and is graphed as a function of $[Ca^{2+}]_0$, $[Mg^{2+}]_0$ and $[Sr^{2+}]_0$ in cell No. 11 from an aestivated (a) and an activated snail (b). Experimental details are described in the text. BPP amplitude is parabolically dependent on $[Ca^{2+}]_0$ and $[Mg^{2+}]_0$ and directly dependent on $[Sr^{2+}]_0$ in both cells. The range of $[Ca^{2+}]_0$ over which BPP activity can be elicited is 1–5 mM in the aestivated cell and 1–60 mM in the active cell. (The interrupted line describing BPP amplitude as a function of $[Sr^{2+}]_0$ indicates that the cells tended to remain depolarised at the peak of the BPP unless hyperpolarising current was injected (see text).) The change in sensitivity of the BPP amplitude to $[Ca^{2+}]_0$ appears to be the major consequence of aestivation. ●, Ca; ○, Mg; ◐, Sr.

In cells from aestivated snails BPP amplitude was parabolically dependent on the external calcium concentration ($[Ca^{2+}]_0$) over the 1–5 mM range (Fig. 2a). In 5 mM $[Ca^{2+}]_0$ the cell was hyperpolarised and could not generate BPP activity in spite of sustained depolarising current. The BPP amplitude was also parabolically dependent on $[Ca^{2+}]_0$ in cells from active snails, but the range over which BPP activity could be elicited was extended to 60 mM $[Ca^{2+}]_0$, which is six times the physiological $[Ca^{2+}]_0$ (Fig. 2b). Transitional cells manifested an intermediate sensitivity to calcium. BPP amplitude was also parabolically dependent on external magnesium concentration ($[Mg^{2+}]_0$) in both aestivated and active cells. The range over which BPP activity could be elicited, as well as the amplitude of the BPP, was smaller in the aestivated group. BPP amplitude was found to be directly related to $[Sr^{2+}]_0$ over the 1–60 mM range in both aestivated and active cells. Little, if any, inhibiting effect was observed at

60 mM $[Sr^{2+}]_0$ in either group of cells. In addition the BPP amplitudes were greater in $[Sr^{2+}]_0$ than in either $[Ca^{2+}]_0$ or $[Mg^{2+}]_0$ although those in aestivated cells were generally smaller than those in the active group. Higher concentrations of $[Sr^{2+}]_0$ (20–60 mM) frequently resulted in a depolarisation of the membrane potential with continual repetitive spiking activity (unlike similar concentrations of Ca^{2+} and Mg^{2+} , which tended to hyperpolarise the membrane potentials of both groups of cells). Under these conditions the BPP cycle could be initiated by a brief pulse of hyperpolarising current and BPP activity maintained by proper adjustment of a sustained hyperpolarising current.

The above observations demonstrate that divalent cations are not only involved in the generation of BPP activity but also in its seasonal modulation. Divalent cations are necessary for BPP activity and at physiological levels (10–20 mM) the relative effectiveness in increasing BPP amplitude was $Sr > Mg > Ca$. At higher concentrations (>30 mM) the reverse order of effectiveness in inhibiting the BPP was found: $Ca > Mg (\ll Sr)$. The results also show that calcium plays an important role in the seasonal modulation of pacemaker activity. The sensitivity of aestivated cells to the inhibitory effects of calcium is increased ten times relative to the sensitivity of active cells. The calcium concentration of the haemolymph of another species of land snail is markedly increased during aestivation^{9,10}. If *Otala* undergoes the same change in haemolymph calcium, there will be more than enough available to inhibit BPP activity (since aestivated cells are inhibited at calcium concentrations physiological to active snails (10 mM)). We are investigating the interaction between the calcium, BPP activity and protein synthesis, since this cell also shows marked changes in the patterns of protein synthesis during aestivation⁷.

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JEFFERY L. BARKER
HAROLD GAINER

*Behavioral Biology Branch,
National Institute of Child Health
and Human Development,
National Institutes of Health,
Bethesda, Maryland 20014*

Received May 30; revised August 31, 1973.

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Possible Involvement of Taurine in the Genesis of Muscular Dystrophy

ALTHOUGH taurine is a major amino acid in muscles and nerves its physiological role is not well understood¹. It has been proposed to be a neuro-transmitter and has been shown to be released from electrically stimulated rat cortical slices² and spinal cord slices³.

The taurine content of skeletal muscle in genetically dystrophic chickens is reported to be higher than in normal chickens⁴. The dystrophic animals excrete this amino acid and its metabolite in the urine^{5,6}. Dystrophy induced by drugs or X-irradiation also enhanced excretion of taurine in urine⁷. Reports have indicated the involvement of a presynaptic defect in muscular dystrophy⁸⁻¹⁰. On the results of cross innervation studies in the mouse, Law and Atwood have proposed that a "trophic" factor which is released from a presynaptic site may be absent in dystrophic muscle⁹.

In our experiments, cats were anaesthetized with 'Dial-urethan' (0.6 ml kg⁻¹, i.p.) and a sciatic nerve-anterior tibialis muscle preparation made. The sciatic nerve was stimulated supramaximally at a rate of 6.4 Hz with square waves of 0.5 ms duration. The contractile force was recorded with a Grass FT03 Force-displacement transducer. Taurine was dissolved in Krebs-Henseleit solution (pH 7.4)¹¹.

When taurine was injected into the femoral vein on the contralateral side from the monitored anterior tibialis muscle, no effect on twitch tension at any concentration (0.1 mm-10 mm kg⁻¹) was seen. It is possible that taurine did not reach the neuromuscular junction, because of its rapid binding to platelets (J. Gorden, personal communication).

Intra-arterial, sequential injections of taurine (1 mmol kg⁻¹) close to the monitored neuromuscular junction, however, produced a dose-dependent decline in the amplitude of the muscular twitch response.

When the sciatic nerve was tetanically stimulated, the depression of muscle contraction was temporarily reversed. Direct skeletal muscle stimulation also reversed the depression of contractility for the duration of the direct stimulation. These studies indicate that taurine depresses transmission at the neuromuscular junction. Studies described here, however, do not adequately pinpoint a presynaptic or postsynaptic action of the drug. Further studies will have to be made to delineate its precise site of action. The depressed neuromuscular transmission caused by increasing taurine concentrations in the vicinity of the neuromuscular junction is compatible with the demonstration of increased levels of this amino acid in skeletal muscle and urine of man and animals with muscular dystrophy.

Because taurine depressed neuromuscular transmission in the cat and changes in taurine metabolism have been reported in muscular dystrophic animals, we propose that changes in this amino acid may be related to muscular dystrophy or may be the biochemical basis of lesions caused by this disease.

S. I. BASKIN*

R. DAGIRMANJIAN†

Department of Pharmacology,
Ohio State University,
Columbus, Ohio 43210

Received July 11, 1973.

* Present address: Department of Pharmacology, Medical College of Pennsylvania, Philadelphia, Pennsylvania 19429.

† Present address: Department of Pharmacology, University of Louisville, Louisville, Kentucky 40201.

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Salting-out in Amphiphilic Gels as a New Approach to Hydrophobic Adsorption

HYDROPHILIC gels such as those formed from cross-linked dextran-Sephadex⁻¹, agar² and agarose³ are extensively used for molecular-size dependent fractionation (such as gel filtration and gel permeation chromatography). Although solute-gel interaction other than, or in addition to, molecular sieving are often evident⁴⁻⁶, such interactions have seldom been deliberately exploited for fractionation of protein mixtures. We describe below a method for protein fractionation based on hydrophobic salting-out adsorption in non-ionic amphiphilic gels.

Amphiphilic gels are produced by introducing a limited number of hydrophobic groups into hydrophilic gel-forming substances, yielding cross-linked polymers which are highly permeable and capable of extensive swelling in water as well as in many organic solvents. In aqueous solutions they show affinity for hydrophobic solutes or solutes with hydrophobic groups. The adsorbents, the use of which we advocate, do not contain ionic groups, in contrast to those obtained by coupling aliphatic amines, tryptophan, tryptophan esters and so on, to cyanogen bromide activated agarose⁷⁻⁹. Non-ionic amphiphilic gels, therefore, should not exhibit mixed ionic-hydrophobic adsorption but, instead, "pure" hydrophobic interaction with hydrophobic regions on the molecular surface of proteins and other solutes. The adsorption capacity for proteins can be very high in certain conditions, providing the fundamental basis for a promising new type of chromatography, which we shall term hydrophobic salting-out chromatography.

The gel used here to demonstrate the new fractionation technique, a benzyl ether of cross-linked agarose, can be prepared as follows: agarose gel of about 6% matrix content, cross-linked with epichlorohydrin or 2,3-dibromopropanol¹⁰, preferably in the form of spherical beads, is swollen in water. To a gel volume of 200 ml including the interstitial water, 200 ml of 5 M NaOH is added, followed by 50 ml benzyl chloride and 1 g sodium borohydride. The suspension is heated to 80°C with stirring and reflux. After 5 h the reaction is discontinued and the product transferred to a Büchner funnel and washed with ethanol and water. The concentration of benzyl groups is estimated at around 1.8 mmol g⁻¹ of dry gel substance.

Table 1 Concentration and Group Separation of Kidney Bean Proteins by Benzylated Cross-linked Sepharose 6B*

Fraction	Total protein (%)	Volume (ml)
A	58	4,000
B	5	60
C	15	120
D	10	150
E	4	20
Intermediate fractions	8	490

* See Fig. 2.

To illustrate the general phenomena, cytochrome *c* was selected as a convenient model substance. Several frontal chromatograms were developed on a column of benzylated cross-linked Sepharose 6B (1.0×19.5 cm) using solutions containing 1 mg of protein ml⁻¹. In one set of experiments the chromatograms were developed with solutions of 0.5 M sodium phosphate buffer (pH 7) containing different concentrations of sodium chloride. Chromatograms were also developed in 0.1 M sodium formate buffer (pH 3) with and without sodium chloride (Fig. 1). Adsorption increases strongly with increasing concentration of salt and is further enhanced by lowering the

pH from 7 to 3. Unsubstituted cross-linked Sepharose 6B does not adsorb cytochrome *c* in these conditions, which clearly demonstrates the importance of benzyl groups for the hydrophobic adsorption.

To illustrate concentration and group separation of proteins a diluted sample of kidney bean extract¹¹, containing 0.8 g of freeze-dried protein dissolved in 4 l of 0.1 M sodium formate buffer (pH 3) and 3 M in NaCl, was introduced into a benzyl agarose column. Elution was performed by increasing the pH, decreasing the ionic strength and lowering the polarity of the solvent (Fig. 2). Trypsin inhibitor activity¹² and haemagglutinin activity were measured on aliquots¹³ (Fig. 2 and Table 1). Phytohaemagglutinins are found in Fractions C, D and E. Fraction E contains 95% of the trypsin inhibitor activity 200-fold concentrated on a volume basis. The purification factor based on adsorbance at 280 nm was 25 for trypsin inhibitor. In an experiment performed as above, except that sodium chloride was omitted from the sample and the starting buffer, only 12% of the protein material was adsorbed to the gel.

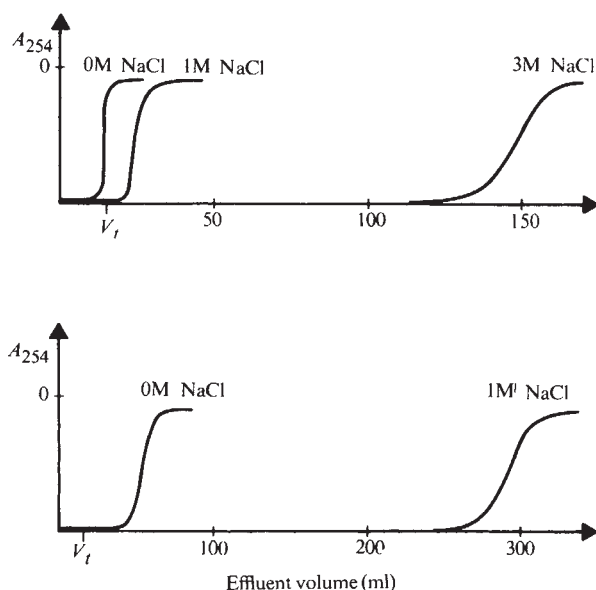


Fig. 1 Frontal chromatograms of cytochrome *c* in 0.05 M sodium phosphate buffer, pH 7.0 (above), and 0.1 M sodium formate buffer (below), obtained with and without sodium chloride included in the solutions. Transmission plotted against effluent volume. Adsorbent: benzyl ether of cross-linked 'Sepharose' 6B. Total volume of the column: 15.2 ml. See also text.

The bed used in these bean extract experiments has been repeatedly used over a period of more than three months. More than 20 chromatograms have been made and the gel has been exposed to amounts of protein corresponding to more than 50 times its own dry weight. No progressive change in its chromatographic properties has been observed. The gel is extremely durable and the adsorption-desorption processes are highly reproducible.

The following conclusions can be drawn from our present experience. (1) At high concentrations of salts and at pH values in the acid range a very high capacity can be attained. For example, benzylated agarose can adsorb amounts of proteins corresponding to several times its own dry weight. (2) High salt concentration often promotes such strong adsorption that, once adsorbed, a protein may be desorbed only by severe reduction in ionic strength and/or lowering of the polarity of the medium and/or elevation of the pH. In contrast to the other chromatographic methods based on agarose containing hydrophobic groups⁷⁻⁹, desorption is thus achieved by decreasing the ionic strength, while in the former

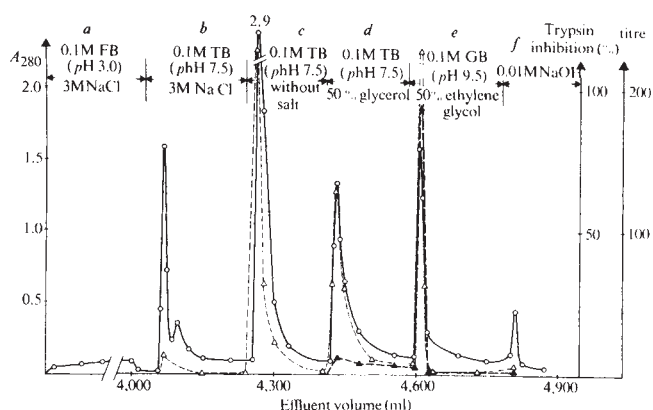


Fig. 2 Chromatogram of kidney bean extract on benzyl ether-'Sepharose' 6B. Column size: 0.9 × 11.5 cm; flow rate: introduction of sample 90 ml h⁻¹; flow rate: elution 18 ml h⁻¹; buffer systems: formate buffer (FB); Tris-HCl buffer (TB); glycine buffer (GB). Trypsin inhibition was measured on 40 times diluted material (5 min incubation with 0.1 mg trypsin ml⁻¹). Absorbance at 280 nm: ○—○; trypsin inhibition (%): ▲—▲; agglutinating activity (titre) of human erythrocytes as inverted dilutions: △ --- △. See also Table 1.

methods desorption is attempted with increasing salt concentrations (which promote hydrophobic adsorption). Furthermore, according to our method, desorption can be accomplished increasing rather than lowering the pH of the eluent. (3) Effective group separations and concentration of protein solutions can be accomplished easily.

The underlying principle of salting-out adsorption is not clearly understood, but presumably the driving force is the entropy gain arising from structure changes in the water surrounding the interacting hydrophobic groups.

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JERKER PORATH
LARS SUNDBERG
NERMIN FORNSTEDT
INGMAR OLSSON

*Institute of Biochemistry,
University of Uppsala,
Box 531, S-751 21 Uppsala*

Received June 11, 1973.

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Reference Abbreviations

ALL abbreviations of references in *Nature* should now conform to the style of the *World List of Scientific Periodicals*, fourth ed. (Butterworth, 1963-65). Authors submitting manuscripts are asked to ensure that the references are written appropriately.

TELEVISION REVIEW

Broadening Horizons

HORIZON, BBC Television's science feature, has eased its way back on to our screens for the autumn with little of the fuss and fanfare surrounding one-off specials like *The Life Game*. With true professionalism, the series has built from a gentle start with Konrad Lorenz and his performing geese followed by the immediate human impact of the problems of spastics, before moving on to the hard stuff of plant genetics and the even harder stuff of black holes. Even granting that Lorenz provided in some way an easy first programme, however, whether by luck or judgment the *Horizon* team can bask in the knowledge that their programme was screened just a few weeks before the man was awarded a Nobel Prize.

Like the series itself, individual programmes seem designed to lead the casual viewer gently into the mysteries of the week's chosen topic; rose gardens, for example, providing an easy lead in to the mysteries of genetics and the possibilities of easing the world food problem by genetic tinkering. This ordered structure is a great aid to the viewer, and the two *Horizon* programmes (*Gilding the Lily* and *The Black Holes of Gravity*) which appeared in the weeks immediately before and after *The Life Game* (reviewed in *Nature* last week) emphasised only too clearly the failings of the latter in this respect. The first beat *The Life Game* at a part, at least, of its own game; the second covered an

equally broad topic more clearly, more entertainingly, in less than half the time and at much less than half the cost.

What were the particular virtues of *The Black Holes of Gravity* which made this programme such a striking example of both good science and good entertainment? Continuity is perhaps the key without which all technical tricks are useless, and the programme, after a quick pre-title flash, began at the beginning of man's fumbling with the secrets of gravity, building from Greek ideas to those of modern relativists. Indeed, black holes were not mentioned after the titles for a full 40 minutes, in a programme only 55 minutes long. Given the logical buildup of the theme, the visual aids available to TV were used imaginatively and helpfully in the context of the theme. Both graphics and models assisted the viewer to understand the theories being propounded, although in one case it did seem that a computer graphic display was used because it was pretty (which it certainly was) rather than because it was informative (which it certainly was not).

With one notable exception, the programme also avoided straying into sensationalism in what is, after all, a pretty sensational subject. For example, Professor Joseph Weber's enthusiastic claims that 200 gravity wave events are being recorded by his equipment each month was matched by Dr Tony Tyson's sober assessment of what is now the general view, that Weber is probably observing something other than gravity

waves. It was a little odd that the possibility of a gravitational "constant" which varies with the age of the Universe was introduced without mentioning Hoyle's work—but on the other hand it was pleasant to see a British-made programme about the frontiers of astronomical knowledge which showed a few different faces, and no doubt that explains the absence of that bastion of astronomy in this case.

The exception to this sober approach unfortunately came right at the end of the programme, and thus left a slightly sour aftertaste. Professor John Taylor, gazing earnestly into the camera, spoke sincerely of the philosophical implications of black holes, and how they will swallow up the Universe so that "in the future there will be no future". Stirring stuff, but he did neglect to point out that this calamity is not due to take place tomorrow, or even in the next 10^9 years, so that we can for the immediate present concentrate on the more mundane problems facing the world.

Even with that caveat the programme stood as a glowing example of why the best of BBC TV science is the best TV science in the world. Perhaps the team responsible for the BBC science epics should, after their recent failure to maintain their own high standards, be made to take a refresher course in the fundamentals of producing entertaining and informative science programmes, by being set to watch a series of the best of *Horizon*.

JOHN GRIBBIN

CORRESPONDENCE

Smoking, Pregnancy and Publicity

SIR,—P. R. J. Burch (*Nature*, 245, 277; 1973) quotes some articles by Professor Yerushalmy which, he claims, support the notion that it is the 'smoker' rather than the 'smoking' which is responsible for the association between low birthweight and smoking in pregnancy. Unfortunately, Yerushalmy's analyses^{1,2} are statistically unsound, a fact I have discussed in detail elsewhere^{3,4}.

Briefly, what Dr Burch describes as "inconsistencies" in the "causal hypothesis" arise from straightforward statistical misunderstandings. The first so-called "inconsistency" is that among low birthweight babies ($\leq 2,500$ g), those whose mothers smoked during pregnancy have a lower perinatal mortality rate and a lower severe congenital anomaly rate than those whose mothers did not smoke. The explanation for this find-

ing simply follows from the (undisputed) fact that the distribution of birthweights for the babies of smokers is the same as that for non-smokers except that it is shifted downwards by about 170 g. It necessarily follows that, for babies $\leq 2,500$ g, the birthweight distribution has a higher mean value for smokers than for non-smokers. It is not surprising, therefore, that the low birthweight babies of smokers fare better than the low birthweight babies of non-smokers.

The second point concerns the fate of babies born to mothers before the mothers started to smoke. In comparing mothers who had never smoked with those who started smoking before they were 25, Yerushalmy unfortunately did not carry out an age standardisation. In particular, the latter group will tend

to have had babies at younger ages and would therefore be expected to have had lighter babies. To my knowledge, Yerushalmy has still not carried out a proper standardisation and until this is done his conclusions cannot be accepted.

It seems to me that the statistical evidence for an association between smoking in pregnancy, low birthweight and perinatal mortality remains strong. The only point which is really at issue is the nature of the mechanism responsible for the association, whether the smoking itself causes the lowered birthweight and so on, or whether some pre-existing characteristics of the woman are responsible for both. Some studies currently in progress, comparing babies born to women who have been actively persuaded to give up smoking during pregnancy with those born to women

who have not, should help us to distinguish between these two hypotheses.

Yours faithfully,

HARVEY GOLDSTEIN

National Children's Bureau,
Adam House,
1 Fitzroy Square,
London W1P 5AH

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³ Goldstein, H., *Am. J. Epidemiol.*, **95**, 1 (1972).

⁴ Goldstein, H., *Am. J. Obstet. Gynecol.*, **114**, 570 (1972).

SIR,—Your editorial (*Nature*, **245**, 61; 1973) raises a central problem in environmental protection: when action is justified on the basis of incomplete evidence. You are correct that the study by Goldstein *et al.* does not prove that smoking during pregnancy causes the deaths of 1,500 babies in Britain each year, but the study does indicate a substantial probability that it does. You may possibly be right that an effective campaign to prevent pregnant women from smoking might cause comparably serious side effects, but the probability that it would is surely much smaller. You place yourself in a curious logical position, because the standard of proof you demand—"that compulsory abstinence produces the same sort of statistics as voluntary abstinence"—could only be obtained by a campaign of the kind to which you object.

A determined campaign to eliminate smoking, started in the early nineteen-sixties, would, if effective, have probably averted at least a million smoking-related deaths. Paraphrasing your arguments, it is "quite possible" that it would also have had undesirable side effects. "Perhaps" involuntary ex-smokers would have died in large

numbers from stomach ulcers, or road accidents, or other ill-defined consequences of "well-known tensions", but the balance of probabilities is and was against this. Those who opposed controls on smoking in the 1960s should now have a heavy burden on their consciences.

The same questions are raised by almost every major environmental health issue, whether sulphur in fuel, lead in petrol, DDT on food crops, or asbestos in beverages. It is never possible to 'prove' hazard completely, because of the host of uncontrolled variables and the lack of unexposed controls. Decisions to act against a hazard must always be made on incomplete evidence. But those who argue for inaction until the hazard is 'proved' bear as much moral responsibility for the consequences of their recommendations as those who urge action before the evidence for hazard is compelling.

In your case, the burden of responsibility is relatively light. The course of inaction which you recommend would probably result in the deaths of only about 1,500 babies a year in Britain, or perhaps 50,000 a year world-wide. As you say, perhaps the effects would be less serious, or even beneficial. But the weight of evidence, as it exists now, is against you. I hope your conscience will remain clear.

Yours faithfully,

I. C. T. NISBET

Massachusetts Audubon Society,
Lincoln,
Massachusetts 01773

A list of books received will appear in the November 2 issue of *Nature*.

Announcements

Erratum

IN the book review 'Electromyography' (on page 437 of this issue) the title of the book should be *New Developments in Electromyography and Clinical Neurophysiology*.

International Meetings

December 2–5, **First International Symposium and Exposition on Ozone for Wastewater Treatment** (International Ozone Institute, 24 Central Avenue, Waterbury, Connecticut 06702, Myron E. Browning).

December 3–4, **The Mammalian Fetus—Comparative Biology and Methodology** (Dr E. S. E. Hafez, Department of Gynecology-Obstetrics, 550 E. Canfield, Detroit, Michigan 48201).

December 3–5, **Advances in Analytical Toxicology** (Registrar, Institute of Clinical Toxicology, PO Box 2565, Houston, Texas 77001).

December 4, **Technology of Superplasticity** (The Meetings Officer, The Institute of Physics, 47 Belgrave Square, London SW1X 8QX).

December 4, **Analytical Techniques for the Museum and Art Gallery** (The Society for Analytical Chemistry, 9/10 Savile Row, London W1X 1AF).

December 4–5, **The Selection of Materials in Machine Design** (Conference Department, Institution of Mechanical Engineers, 1 Birdcage Walk, London SW1H 9JJ).

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